THE MATHEMATICAL GAZETTE

EDITED BY
W. J. GREENSTREET, M.A.

WITH THE CO-OPERATION OF
F. S. MACAULAY, M.A., D.Sc., F.R.S.
AND
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GRAVITATION.*

BY PROF. S. BRODETSKY, M.A., PH.D.

To our modern minds it seems obvious that scientific progress must be based upon alternate induction and deduction. The first inductive step is, indeed, not always evident. It was only in the nineteenth century that the emergence of non-Euclidean Geometry made it clear that even a purely deductive science like Geometry must be based upon a preliminary induction—often subconscious, and the result of generations of tacit acceptance of intuition. Modern classical dynamics commences with the Galilean induction in the form of Newton's Laws of Motion, thus discarding the old Greek view of motion and its causes. In some branches of human knowledge, *e.g.* the sociological, the first induction is hardly yet accomplished.

There is thus considerable difficulty about the first induction. The term "common sense" is often applied to it, so that it acquires a halo of logical obviousness which it does not deserve, which it should not claim, and which is indeed quite irrelevant to its very essence. It is not only the layman who thus misuses the term "common sense"—the scientist, too, cannot always escape the temptation to misuse it—with ludicrous results. Common sense is surely the result of common experience, induction based upon popular intuition. This induction must necessarily be defective since our human experience is very limited and our powers of reasoning are liable to error. Common sense, therefore, needs continual revision. The apparent absurdity of to-day becomes the common sense of to-morrow: we look back with amused incredulity at what was universally accepted as incontrovertible truth only a few generations ago, and we are no doubt laying up a store of ridicule for the delectation of posterity at our own expense.

Every great scientific advance is therefore a breaking away from more or less hallowed tradition, and requires a powerful wrench for its achievement. To convert the flat earth into a spherical globe, to make this globe turn on its axis, and revolve round the sun, instead of standing still majestically at the centre of all things, to make the sun move through space instead of occupying the throne it usurped from the earth, to see an equal rate of fall of all bodies towards the earth when obviously the heavier bodies fall faster than the lighter ones, to see time and space wedded in an indissoluble union yet with infinite variety of aspect, instead of the time and the space existing side by side in jealous independence with yet mysterious dependence—these and other revolutions in scientific thought have required emancipation from tradition and struggle for intellectual freedom, often mirrored painfully in human history.

* A lecture delivered at the Annual Meeting of the Mathematical Association, Jan. 5, 1928.

We have made great progress in this respect. Whereas the scientific rebel of the Middle Ages suffered an unpleasant notoriety, the scientific rebel of to-day is often acclaimed merely because he is a rebel. Not only is there unprecedented freedom of thought in modern civilised life, but also the great expansion of the domain of science, and the intense specialisation that is essential for progress in each branch, make the layman in any branch of science accept without questioning the results announced by the specialist in the branch—not always with complete justification.

The fact is that the average person divides science roughly into two kinds, the kind that matters to him and the kind that does not matter to him. He cannot, of course, make this division rationally, since he can never tell what may and what may not affect his personal interests or fortunes. The sun-begotten helium turns out to be a substance of national interest and international jealousies. Yet the average person does not realise this. He sees in astronomy something interesting but innocent. He sees in mathematics a juggling with figures and symbols, the conclusions of which are either the inane $x=0$ or the absurdities of flat-land and of four-dimensional space. In these subjects all results are accepted readily and without unpleasantness. To a certain degree one favours the bizarre and the unexpected, as if the astronomer and mathematician were spoilt children whom one humours so long as their antics lead to no serious harm.

But the average person will not accept unprotestingly results of a different kind. The theory of the evolution of species touched his piety and his vanity, and he objected violently—he still does in some places. The theory of relativity mystified him: he was suddenly called upon to think in terms of a mixture of space and time, to see in time an imaginary fourth dimension of space, to accept views that ran counter to the accepted notions of all ages since prehistoric days. He felt that his intellectual foundations were being taken from under his feet and placed above his head.

We can therefore appreciate the extraordinary difficulty that the human mind experienced in gradually framing a rational view of the universe. Primitive observation showed the stars going round the earth, with exceptions called planets: this produced the first induction, namely of a flat earth with the heavenly bodies going round it in various periods. Gravitation referred only to earthy matter, and "up" and "down" motions occasioned by gravity hardly had physical significance. Then came the great revolution in the discovery that the earth is spherical, and finally there emerged the theory associated with the name of Ptolemy in the second century A.D., a geometrical and not altogether inaccurate picture of the motions of the heavenly bodies round the earth. But speculation about the causes of these motions yielded nothing of value. The best that ancient Greece could say about the matter was that the stars and planets were attached to spheres which rotated round the earth, and the oriental minds of Arabs and Jews added ornamental detail.

No dynamical basis existed for this speculation—no rational dynamics existed at all till the seventeenth century. These spheres made no lasting impression on science, since they did not lead to deduction and verified prophecy. The spheres, their radii, thicknesses, positions and rotations were postulated *ad hoc* for each special case—there were no fundamental ideas or relationships, except that all the fixed stars were attached to the same sphere. Eventually the spheres were discarded and celestial physics really disappeared. Instead, gratuitous assumptions were made for which no physical basis existed—that heavenly bodies must move in circles with uniform speeds. As to gravitation, this still remained a property of earthy matter only, and its direction, always passing through the centre of the earth, *i.e.* the centre of the universe, gave it a character more geometrical than physical.

In the old Greek view the stars were somehow dragged round the earth once a day, and the planets were also dragged round the earth in their respective periods. This crude conception gave way to a sort of spiritual compulsion, circular motion of celestial matter. Nicolaus Copernicus (1473-1543) released the fixed stars completely: they were no longer dragged physically or spiritually round the earth, but the earth rotated once a day instead. The planets were not liberated—they were handed over to another bondage, that of one of them, the sun, and the earth was degraded into servitude.

New ideas had to accompany this change. We live on the earth and can see that there is nothing forcing it round on its axis or round the sun. Clearly a uniform rotation of the earth round its axis can go on indefinitely without any physical cause—here we have a suggestion of the basis of modern dynamics. If the earth moved exactly in a circle and with uniform speed round the sun, then we might say similarly that uniform motion in a circle needs no cause for its maintenance—and this heritage of Greek science Copernicus duly incorporated in his system. But in actual fact the earth and the other planets do not move in exact circles round the sun, the moon does not move in an exact circle round the earth. There are deviations from the exact circle and from uniformity of speed: these matters were quite beyond Copernicus' dynamics. His treatment, *e.g.*, of a simple harmonic oscillation on the basis of uniform circular motions, indicates the tremendous difficulties in the way of really effective development.

If Copernicus could not explain these deviations, he had a remarkable premonition of what turned out to be the correct explanation. All bodies near the earth are seen to fall towards the earth, except celestial bodies like the sun, moon, etc. In Greek science this was in complete harmony with the view that the earth is at rest at the centre of the universe, and the law of gravitation was that all non-celestial or earthly matter naturally tends towards the centre of the universe, the heavier bodies falling faster and the lighter bodies lagging behind. But to Copernicus the earth was no longer the centre of the universe—yet bodies near the earth fell towards the earth's centre in Copernicus' as in Aristotle's day. Copernicus was therefore forced to conclude that the falling of all bodies towards the earth was not a property of the universe, but a property of the earth—and by obvious extension a property of all the planets, and why not of the sun, too? To Copernicus, therefore, the earth's gravity became an inherent tendency of all matter to congregate together in the form of spheres, and in 1543 the first rational physical idea about universal gravitation was given to the world. But neither Copernicus nor his immediate followers could make anything of the idea, since dynamical principles and mathematical tools were lacking for such an enterprise.

The English physician, William Gilbert (1540-1603), was a pioneer in the study of magnetism and electricity. He established the magnetic nature of the earth, and was naturally led on to consider the forces that drag the moon with the earth round the sun. In 1600 he suggested that the earth attracts the moon like a lodestone. Gilbert could not explain why the earth and moon did not fall together—this was not possible till Galileo laid down the foundations of dynamics long after Gilbert's death. Gilbert died in 1603, but in a posthumous work printed in 1651 he ascribed to the moon, too, magnetic properties similar to those of the earth, attributing the tides to the magnetic effects of the moon.

Meanwhile considerable improvement was taking place in our knowledge of the actual motions in the solar system. The observations of Tycho Brahe (1546-1601) had enabled John Kepler (1571-1630) to discover a more exact statement of the motions of the planets. In 1609 Kepler published his first two laws of planetary motions, and in 1619 he published the third law. These laws form the basis of all modern study of the motions of the planets; they are:

I. Every planet moves on an ellipse with a focus at the centre of the sun.

II. The line joining the centre of the sun to the centre of the planet sweeps out equal areas in equal times.

III. The square of the time taken by a planet to describe its path once completely round the sun is proportional to the cube of its mean distance from the sun.

Kepler's remarkable success with the geometry of the solar system led him to search for the dynamics of these motions. Naturally he failed, for no dynamical theory was possible till the principles of dynamics were known. But Kepler guessed at the cause, namely gravitation, and he even postulated a universal gravitation acting between any two pieces of matter. He overcame Gilbert's difficulty about the earth and moon falling together by assuming the existence of a force which regulated the distance between them, and exhibited his ignorance of dynamics still further by attributing the continued motion of a planet to a force at the planet perpendicular to the radius vector from the sun, due to a magnetic effect set up by the sun's rotation! The subject was beyond the powers of even the great Kepler—his ideas on gravitation were mere, and comparatively wild, speculation.

Galileo Galilei (1564-1642) was the first man to achieve success in dynamics. He discovered the principles which are set forth in the first two of Newton's laws of motion, and thus founded modern dynamics. Nevertheless, Galileo failed to make any progress in the explanation of the heavenly motions. He accepted the attraction of the moon by the earth, but was so old-fashioned as to refuse a similar privilege to the moon as attracting the earth: the third law of motion, namely that action and reaction between any two bodies must be equal and opposite, was not enunciated till 1687, when Newton published his *Principia*. It must be admitted that Galileo left the problem of the motions of the planets in no better condition than he found it.

Nevertheless, progress would no doubt have been made very soon, since the dynamics of Galileo contained the clue to the whole matter, were it not for a reaction that set in due to the influence of the greatest philosopher of that generation. René Descartes (1596-1650) caused a temporary halt in the progress towards a gravitational view of the universe by means of his theory of vortices. A popular feature of the oldest Greek view of the universe was that the heavenly bodies were dragged round in a manner that anybody could grasp. Gravitation applied only to non-celestial matter, and was in fact a proof of the lowly origin of matter affected by it. Copernicus, Gilbert, Kepler and Galileo turned to gravitation in order to explain the motions of the planets, but there were obvious difficulties. Why should a radial force be associated with motion mainly perpendicular to the radius? Why do not gravitating bodies fall together? What is the mechanism or the physical agency involved in gravitation between bodies far apart?

Descartes went back to the old Greek physical view. Imagine that space is filled with an ocean of "ether" and let there be whirlpools in the ether. The planets, caught in such a vortex round the sun will be dragged round it as pieces of flotsam are dragged round a water whirl, or dust particles in the small cyclones we witness in daily life. Let there be small whirlpools round the planets, and satellites will be dragged round them as observed.

The theory was realistic and easily intelligible. These advantages in an age when men were becoming more and more intimately acquainted with physical nature and its workings, combined with the great reputation of Descartes himself, gave it universal approval. It was published in 1644 and was accepted by all except a very few till about a century later, although Newton gave it its death blow in 1687. Its faults were serious and obvious. In a whirlpool as postulated the speed should increase as one goes outwards, while in the solar system the exact reverse was the case: Mercury goes round the sun faster than Venus, Venus travels faster than Mars, etc. Further, why were the paths not exactly circular? How could the paths of comets be

explained—since they were shown by Tycho Brahe to come quite close to the sun and then go off to very great distances ?

For a generation the theory of vortices was unchallenged. Giovanni Alfonso Borelli (1608-1679) found experimentally that if a body is whirled round in a circle it tends to fly outwards. This is of course in accordance with the principles discussed by Galileo, and, as we shall soon see, was used by several men in connection with planetary motions. Borelli was the first to argue that, if an inward force is exerted on such a body equal to the outward force due to its circular motion, then the body will continue to move round and round, as if balanced and floating on a surface. In 1666 Borelli attributed planetary motions to an attraction exerted by the sun, and the motion of the moon to the attraction of the earth. But the problem was beyond Borelli's powers, and greater men had to take it in hand before it could be solved.

These men were Robert Hooke (1635-1703), Christopher Wren (1632-1723), Edmund Halley (1656-1742) and Isaac Newton (1642-1727), all Englishmen, the first three being Oxford men living in London, and the last a Cambridge man carrying out all kinds of researches at Trinity College, till he was translated to the Royal Mint in 1696. The first step taken was to discover the exact force required to keep a body moving uniformly in a circle. A clear statement on this point was made in 1671 by Christian Huygens (1629-1695), who proved that the acceleration is directed towards the centre of the circle and equal to $\omega^2 r$, where r is the radius of the circle, ω the angular velocity. Huygens did not apply this result to the planets. Wren, Hooke and Halley all did so within twelve years of the publication of Huygens' result. Newton must have obtained Huygens' result independently, since in 1666 he deduced the inverse square law of gravitation from Kepler's third law.

This law states that the periodic time T of a planet round the sun is proportional to $a^{\frac{3}{2}}$ where $2a$ is the major axis of the path of the planet. Suppose that the path is circular, then the radius is a. The angular velocity ω is $2\pi/T$, and so the acceleration $\omega^2 r$ is proportional to a/T^2. But T^2 is proportional to a^3 ; hence the acceleration is proportional to $1/a^2$, and the force exerted by the sun on unit mass of a planet varies inversely as the square of the distance of the planet.

Wren, Hooke and Halley were able to argue this out for themselves, but could not go any further. Newton did not at once proceed to deduce the necessary consequences of this result. He is traditionally believed to have been held up by the failure of a piece of arithmetical calculation. We are told that seeing an apple fall in the orchard near his house at Woolsthorpe in Lincolnshire, whither he had gone because of the Great Plague which threatened Cambridge, Newton wondered whether the force which makes the apple fall is the same as the force that makes the moon go round the earth. Having first established the inverse square law, Newton argued that the acceleration of the moon to the earth should be gR^2/r^2, where R is the radius of the earth and r is the distance of the moon from the earth, g being the acceleration due to gravity at the earth's surface. But the acceleration of the moon is $\omega^2 r$ where $\omega = 2\pi/T$ and T is the length of the sidereal month, *i.e.* the time taken by the moon to go round the earth relative to the fixed stars. Hence the acceleration is $4\pi^2 r/T^2$. If we put $r = nR$, so that $\sin^{-1} 1/n$ is the moon's horizontal parallax, we get the two values

$$\frac{g}{n^2}, \quad \frac{4\pi^2 nR}{T^2}.$$

T and n had been known with some accuracy for many centuries; R, the radius of the earth, is not an easy thing to measure. Apparently Newton in exile was deprived of the use of books and did not know the accurate value of R, although it was known for many years. He therefore used the rough and ready value of R which corresponds to 60 miles per degree of latitude

on the earth's surface. He found a difference of about 16 per cent. between the two values—and abandoned the idea.

As Newton was interested mainly in chemical and optical experiments, he did not take up the matter again for many years, although the correct size of the earth must have become known to him at Cambridge. Even the excellent measurement of the earth's radius by J. Picard, printed in the *Philosophical Transactions of the Royal Society* in 1675, which Newton must have seen, did not produce any reaction in his mind.

In 1677 Hooke became Secretary to the Royal Society, and Newton communicated to him a method of proving visually the diurnal rotation of the earth. If a body is dropped from A, which is vertically above the point B on the earth's surface, then since A is further from the earth's axis than B, and so moves faster, it follows that the falling body will reach the earth a little to the east of B.

Hooke had thought much about these matters. As early as 1666 he had sent to the Royal Society a paper on the variation of the earth's gravity with height above its surface, making the important suggestion that the force of gravity could be measured by means of the pendulum. In the same year he suggested vaguely that planetary motions could be explained by means of an attraction towards the sun. In 1674 Hooke quite definitely postulated universal gravitation, but he did not see what law to adopt—he had not yet deduced the inverse square law from Kepler's third law of planetary motion.

Having a clear insight into the whole question, Hooke pointed out to Newton that a slight modification was called for in his statement. The motion of the falling body must be in the plane which contains the vertical BA (for a spherical earth this is the radius OB), and the direction of the velocity of A relative to B. This plane touches the cone formed by the circle of latitude at B, and is in fact determined by the great circle which touches at B the small circle of latitude defined by B. It is thus clear that the point where the body reaches the ground cannot be exactly east of B, since east and west are defined by the circle of latitude. The body must deviate a little towards the equator, *i.e.* to the south in the northern hemisphere, to the north in the southern hemisphere. On the equator itself the body reaches the earth exactly east of B.

While Newton and Hooke were discussing this matter the question arose between them as to the exact path of a falling body, taking the earth's attraction to vary inversely as the square of the distance from the earth's centre. Newton suggested a spiral curve, while Hooke asserted that the path would be a conic with a focus at the earth's centre. Newton was thus made to re-examine the problem of the inverse square law. He soon proved that the path is a conic with a focus at the centre of force. Soon afterwards he corrected the error in the Woolsthorpe calculation. In January 1684, Wren, Hooke and Halley met, and found that they had all deduced the inverse square law of gravitation from Kepler's third law, on the basis of exactly circular planetary orbits: but as regards the more important problem of finding the general path under such an attraction, Wren and Halley admitted defeat, while Hooke claimed to have a solution. This was not produced for several months, and Halley visited Newton in August 1684. By November, Newton's solution was in Halley's hands. The inverse square law of gravitation was launched upon its wonderful career of astronomical and intellectual triumph.

In December 1684, Newton began to write his great work. Halley had it printed at his own expense, and did everything possible to give to the world the greatest work of science ever written. The *Philosophiae Naturalis Principia Mathematica* appeared in July 1687. Within half a century Descartes' theory of vortices was dead. Newton's law of universal gravitation based on the inverse square law was ere long accepted everywhere, and acclaimed as the solvent of all nature's problems.

Whatever reluctance there remained in the minds of some philosophers to accepting Newton's philosophy was based upon the notion that it rested upon non-physical ideas. The chief accusation against Newton's gravitation was that it postulated "action at a distance." The fact is, however, that Newton never conceived of gravitation as other than a purely physical agency, somehow created in the ether of space. It is true that he did not in the *Principia* produce an explanation of how gravitational forces between sun and planets, and between planets and satellites, are produced: yet we know that he speculated on the cause of gravitation, and in the queries to his "Optics" we have the famous enunciation of an ethereal theory of gravitation which has given rise to so much speculation since. Newton conceived all space to be permeated by an elastic medium or ether, which is capable of propagating vibrations in the same way as air propagates vibrations of sound, but with greater velocity. It pervades the pores of all bodies and is the cause of cohesion. Its density varies, being greatest in interplanetary spaces and smallest near matter: hence pressures are produced on pieces of matter, pushing them towards one another and causing gravitation.

We know that Newton's genius was due to a remarkable combination of mathematical ability with a practical sense: he was essentially an experimental philosopher rather than a theoretical speculator. His view of gravitation was therefore bound to be physical. Further, the spirit of the age was distinctly in favour of a physical explanation of the motions of the planets. The human mind, in fact, seems to oscillate between two attitudes towards the phenomena of nature. On the one hand, we possess an innate desire for a mechanistic interpretation, for the construction in our minds of a mechanical apparatus, a material model, which we can readily grasp and which gives the results desired. On the other hand, we possess an equal innate desire for a geometrical interpretation, without mechanical models but based rather on what may perhaps be called spiritual or even mystical elements—in other words, we are content with a set of mathematical equations which reproduce the observed facts, and do not demand a more definite picture of what is taking place. Would it be so very far-fetched to suggest that we see in the history of astronomy a continued struggle between the two frames of mind, between the mechanical or physical interpretation of the universe and the mathematical or geometrical interpretation of the universe? Primitive man made a mechanical model in which the stars and planets were dragged round the earth in an obvious and crude manner. This model broke down when the motions of the planets—forwards and backwards with stationary points in between—became better known, and a purely geometrical picture was substituted, one in which the only quasi-mechanical element was the somewhat mystical statement that celestial matter must move uniformly in a circle. This conception prevailed throughout the long centuries from Ptolemy to Copernicus. Then came the great development in natural science. Magnetism was developed as an experimental study. Dynamics became a matter of experimental study. The geometrical theory no longer satisfied—a mechanical model was required. For a time, Descartes' vortex model supplied this need. Then the Newtonian theory supplied a physical basis for a mechanical model—although to many critics the physical was not so obvious in the law of universal gravitation. It will be the theme of the sequel to suggest that the relativity theory of gravitation is another reversal, consequent upon dissatisfaction with the mechanical models invented during three centuries of effort, and with the numerical results of the Newtonian theory itself.

An interesting light is thrown on Newton's attitude towards natural phenomena by the fact that he insisted upon making each element of mass in the sun or in a planet attract in accordance with the inverse square law of gravitation, and was not content to consider the whole sun, the whole earth, or any

other heavenly body as one whole, to be endowed with such a gravitational force varying inversely as the square of the distance from its centre. This gave Newton additional trouble, since he had to prove that a symmetrical sphere of matter produces at external points an attraction equal to what would be produced by a particle of mass equal to the whole mass of the sphere and located at its centre. But the additional trouble was not wasted. On the contrary, the flattened shape of the earth, the theory of the tides and the theory of precession were the rewards that Newton reaped.

When Newton died he bequeathed to humanity a treasure that perhaps no other mortal has left behind him. But two items in this legacy are of particular interest in connection with our subject. As consequences of the law of universal gravitation there emerged two fundamental tasks, which Newton himself undertook, but which were beyond his unaided powers. Great mathematical and physical developments were yet needed in order that these tasks might be undertaken and fulfilled. The first was to work out in all possible detail the consequences of the inverse square law of gravitational attraction, and to compare the results obtained with those given by observation. The second was to construct a satisfactory physical theory of the nature of gravitation. The third book of the *Principia*, especially in second and third editions, is Newton's own and wonderful contribution to the first task. The outline of a theory of gravitation mentioned above is his somewhat meagre contribution to the second task.

The first task is that of celestial mechanics or dynamical astronomy. The greatest minds that humanity has produced have taken their share in the work. Lunar theory came first, since the deviations of the moon's motion from Kepler's laws were obvious even with the comparatively rough observations of the seventeenth century—some of these deviations were known indeed twenty-one centuries ago. Newton started the study of the motion of the moon: in the middle of the eighteenth century A. C. Clairaut (1713-1765) and J. le R. D'Alembert (1717-1783) made significant progress. About the same time Euler began planetary theory, *i.e.* the study of disturbances produced in the motions of any one planet due to gravitational interference from other planets. J. L. Lagrange (1723-1813) followed soon—then came the greatest of all, P. S. Laplace (1749-1827), followed by S. D. Poisson (1781-1840), K. F. Gauss (1777-1855), etc. The discovery of Uranus and of the minor planets paved the way for the sensational and romantic discovery of the planet Neptune by J. C. Adams (1819-1892) and U. J. J. Leverrier (1811-1877) about the middle of the nineteenth century—a remarkable vindication and verification of the law of gravitation. In lunar theory additional names occur like P. G. D. de Pontécoulant (1795-1874), C. E. Delaunay (1816-1872), P. A. Hansen (1795-1874), leading up to the remarkable new methods of G. W. Hill (1838-1916) and the work of E. W. Brown (1866-). Tidal theory gives the names of Lord Kelvin (1824-1907), Lord Rayleigh (1842-1919), G. H. Darwin (1845-1912). Cometary motions are for ever associated with the name of Halley, while many observers and mathematicians have worked on the subject ever since the first confirmation of Halley's prediction of the return of his comet in 1759. Satellite theory, the shapes of the planets—including the earth, Saturn's rings, minor planets, evolution or capture of satellites: these and other applications of the inverse square law have been worked out and have added strength to the Newtonian philosophy. It is impossible to go into full detail here, but it suffices to say that an absolutely astounding and overwhelming volume of approval of Newton's law of gravitation has been obtained. One or two voices of disapproval have been raised from time to time, but these tones of disapproval have almost always been converted into triumphant approval. It is impossible, indeed, to give an adequate impression of the remarkable triumph of the law of gravitation—"the grandest generalisation ever made in science."

Yet one or two tiny voices of discontent have persisted and have refused to be silenced. When the mathematical methods of Lagrange, Laplace and Poisson had become available, Leverrier and others applied them to all known motions. Certain minor discrepancies resulted, so small that they need hardly be taken very seriously for the present. But one discrepancy emerged of somewhat startling magnitude, and for nearly a century the discrepancy persisted. In order to make the matter intelligible it is necessary to say a few words about the manner in which such matters are calculated.

The direct way to obtain the motion of a heavenly body is to represent its position and velocity at any moment in terms of some frame of reference, its accelerations being given by the various forces acting upon it. This method has been found very useful in lunar and in cometary theory. In planetary theory, however, this simple and direct method is not the best. The method of variation of parameters is found to be most useful. The idea is as follows.

Let us imagine some *type* of curve, *e.g.* an ellipse with one focus at the centre of the sun. We only postulate that the curve is an ellipse and that its focus is at the sun: we do not lay down any definite ellipse. In order to define any one ellipse (with its focus at the centre of the sun) we have to give the following data: (*a*) the plane (through the centre of the sun) in which the ellipse lies, (*b*) the shape and size of the ellipse, (*c*) the position of its major axis in the plane of the ellipse, (*d*) where the planet is on the ellipse at some specified time. Now (*a*) requires *two* quantities, since it is really the direction of the normal to the plane that we want, and the direction of a straight line in three dimensions is determined by two quantities; (*b*) requires *two* quantities, the shape being given by the eccentricity, and the size by the length of the major axis; (*c*) requires *one* quantity; (*d*) requires *one* quantity. Thus, in all, *six* quantities will define the ellipse.

But if we consider the planet at any moment, it has three coordinates of position and three components of velocity: there are thus *six* quantities available at any moment, and these six quantities define an ellipse with focus at the sun's centre. If the planet is not liable to any perturbations due to the gravitation of other planets, etc., then the ellipse obtained is always the same one—this is, in fact, Kepler's first law of planetary motion. But in actual fact each planet is disturbed; the consequence is that the ellipse obtained at any moment is not the same as the one obtained a little time before, and not the same as will be obtained a little later. The mathematical study of planetary theory consists largely in the calculation of the ellipse from time to time, *i.e.* in the calculation of the values of the six quantities obtained from (*a*), (*b*), (*c*), (*d*) above—the so-called six elements or parameters of the orbit.

A little of this was familiar to Newton—he proved in fact that certain perturbations produce changes in the plane of the orbit and in the direction of the orbit in the plane. But the elaborate working out of the whole subject came a century after Newton's work.

The six elements are defined as follows.

(*a*) Take a fixed plane through the centre of the sun. Any other plane is defined by the direction of its line of intersection with the fixed plane, and the angle it makes with this plane. Taking a fixed direction through the centre of the sun, lying in the fixed plane, then the angle between this fixed line and line of intersection—line of nodes—is called the longitude of the node, Ω. The angle that the plane of the supposed elliptic orbit makes with the fixed plane is called the inclination, i.

(*b*) The major axis is defined by its half, a. The eccentricity is called e.

(*c*) The direction of the major axis in the plane determined by (*a*) is given by the angle between the line of intersection of this plane with the fixed plane, and the line from the centre of the sun to the perihelion of the supposed

elliptic orbit. This angle, when added to Ω, gives what is called the longitude of perihelion, ϖ: hence the actual angle itself is $\varpi - \Omega$.

(*d*) The position of the planet in its supposed elliptic orbit at a specified time is given by means of the time when the planet would be at perihelion if the supposed orbit were the permanent one, T. It can also be given by means of the direction of the planet as seen from the centre of the sun at some zero time, again assuming that the supposed elliptic orbit is the actual and permanent orbit. Let the angle that this direction makes with the line of nodes be added to the longitude of the node, Ω: the result is then called ϵ. Thus $\epsilon - \varpi$ is the angle from perihelion.

We thus have six quantities, a; e; i; Ω; ϖ; T or ϵ. Differential equations can be found containing these quantities and their first differential coefficients with respect to the time. The task is thus reduced to solving these differential equations. All these quantities vary from time to time in the case of any given planet. The rates of variation are exceedingly slow, but they are important to obtain with as much accuracy as possible, since modern astronomical observations are exceedingly accurate.

The variations are of two types, periodic and secular. The former type consists of variations that oscillate about and on the whole produce no permanent result: the latter type consists of variations that cause continually cumulative effects from century to century. The important cumulative effects are in the quantities Ω and ϖ, *i.e.* in the line of nodes and in the direction of the perihelion as seen from the centre of the sun. The first means that the plane of the supposed elliptic orbit is continually turning round and round, so that its line of intersection with the fixed plane rotates continually in the same sense. The second means that the major axis of the supposed elliptic orbit rotates continually in the same sense in the plane of this orbit. On the whole, we can say that there are no appreciable secular changes in a, e and i, although the periodic variations may be quite noticeable.

Let $\delta\Omega$ and $\delta\varpi$ be the rates of change in Ω and ϖ per century. In the first place it is clear that $\delta\Omega$ does not produce its full effect geometrically in any very obvious manner: the actual distance of a point in the new plane from the old plane depends upon $\sin i \,.\, \delta\Omega$. In the second place, it is clear that $\delta\varpi$ does not produce its full effect geometrically in an obvious manner: the actual distance of a point on the new ellipse from the old ellipse depends upon $e \,.\, \delta\varpi$. It is therefore these quantities that are calculated and then compared with observation. Taking the four inner planets, Mercury, Venus, Earth and Mars, we find that there is no discrepancy in the rates per century for either quantity to the extent of even one second of arc per century, except in the case of the quantity $e\delta\varpi$ for the planet Mercury, the difference between the observed and the calculated rates being the surprisingly large amount of $8\frac{1}{2}$ seconds of arc per century! Since the eccentricity of the path of Mercury is about 0·2, we get the discrepancy in $\delta\varpi$ to be about 42 seconds of arc per century.

Although in actual fact it means that the perihelion of Mercury goes round the sun once more in fact than in theory in the course of about three million years, nevertheless the discrepancy is most serious. If it cannot be accounted for in some legitimate and satisfying manner, then there is something seriously wrong with our theory of celestial mechanics. We thus see that the first task mentioned above in the legacy of Newton takes the form of the urgent need for some explanation of the discrepancy in the motion of the perihelion of Mercury.

Although the first task did finally leave an outstanding difficulty, yet nobody could be so foolish as to deny that on the whole the task had been fulfilled with remarkable success. The wonderful accuracy of calculations based upon the inverse square law of gravitation has become proverbial, and the laity work themselves up into a great heat of excitement if at a total eclipse of the sun the time of totality as calculated in advance is two or three seconds out.

The second task in the legacy of Newton was not fulfilled in any such gratifying manner. It is true that potential theory as developed in the nineteenth century did remove the greatest objection raised to universal gravitation, namely that it connotes action at a distance. It became clear that the gravitational effects produced at any point can be expressed as a function of position in a gravitational field. The force per unit mass being called the intensity, this intensity vector can be assumed to exist at all points, whether there is any matter there or not, and to satisfy certain equations associated with the names of Laplace, Poisson and Gauss. Yet this is perilously like giving up the conception of gravitation as something physical and replacing it by a purely geometrical conception. For a long time the issue was not faced in this sense.

The legacy of Newton thus gave to the eighteenth and nineteenth centuries the problem of the physical nature of gravitation, and to the nineteenth century the problem of the discrepancy in the motion of the perihelion of Mercury. It is useful to examine briefly the chief attempts made to solve these problems.

The attempts to explain gravitation fall mainly into three types. The first is based upon the ether as a fluid. Newton, as we have already seen, speculatively suggested that variations exist in the pressure inside this fluid, the pressure being greater the further away one is from matter. If we imagine two pieces of matter near one another, it is immediately obvious that they are forced towards one another, and a gravitational effect is apparently produced. Many variants of this theory have seen their day and disappeared. The most important are those associated with Robert Hooke, who supposed that the molecules of the ether oscillate and hence produce gravitative effects between the pieces of matter embedded in it. At the end of the nineteenth century a form of this theory was developed by V. Bjerknes (1862-), who supposed matter to consist of pulsating spheres in an incompressible ether. Two such pulsating spheres, whose radii are small compared to their distance apart, having the *same period and phase* in their pulsations, produce via the fluid ether a mutual attraction varying inversely as the square of the distance, and directly as the mass of each, mass being defined as the intensity of pulsation. It would seem then that all the molecules in the universe must pulsate with the same period and phase! We pass on to the second type of theory of gravitation.

In the second type the explanation is again based upon the ether, but this is now taken to consist of a vast number of tiny corpuscles, all in motion. The most important advocate of this type of explanation was G. L. Le Sage (1724-1803), who developed it at the end of the eighteenth century. Let us suppose that all the corpuscles have the same size, that they occupy all empty space uniformly, and move about in all directions with the same speed. Imagine them to be absolutely inelastic. Now let two bodies of matter, large compared to the corpuscles, be near one another. The bangs of the corpuscles will clearly force the bodies towards one another, and it can be calculated that the force varies inversely as the square of the distance between the bodies. Further, let the bodies be very, very porous: then the force can be made to vary as the mass of each.

The obvious objection can be made that we cannot really imagine the speeds of the corpuscles to be equal. This objection can be overcome by adopting, say, a Maxwell distribution of velocities as in the kinetic theory of gases. But other and more serious objections can be raised. What happens to the energy that is lost by collision all the time? What happens if a third body gets between our two bodies? What happens to the resistance which a body is bound to experience in moving through such a medium? These and other objections can be said to dispose of the theories of the second type. All kinds of artificial assumptions may be postulated, but the results are always unsatisfactory.

The development of electrical theory in the nineteenth century had its effect upon our subject too, and produced the third type of theory of gravitation. There are fashions in science as there are fashions in dress. Nearly a century ago it was suggested that there are two kinds of corpuscles, "ether" corpuscles and "matter" corpuscles; that two ether corpuscles repel one another, that matter corpuscles repel one another, but that an ether and a matter corpuscle attract one another, the attraction being more intense than either of the repulsions. Half a century later this became modified into the view of atoms of matter as consisting of positively and negatively charged corpuscles, the attraction between positive and negative being greater than the repulsion between positive and positive or between negative and negative. A residual gravitation between two atoms is thus obtained. The theory was worked out by W. E. Weber (1804-1891), and others. H. A. Lorentz (1853-1927) returned to this problem at the end of the nineteenth century. But the object which the followers of this theory had in view was the obtaining of a method of dealing adequately with the problem of the motion of the perihelion of Mercury—and in this respect the electrical theory of matter, like other attempts to deal with Mercury, failed, as we shall see.

When the discrepancy between the motion of the perihelion of Mercury as calculated by the inverse square law of gratitation, and as obtained from observation of the planet in the sky, became an established fact, attempts were immediately made to explain the discrepancy. These attempts, too, fall into three main types.

The first type is another interesting illustration of the influence of fashion in science. The discovery of the first four minor planets at the beginning of the nineteenth century was followed before very long by the discovery of many more. Leverrier himself, and independently of him, Adams in England, discovered the new planet Neptune on paper, by an examination of the outstanding discrepancies in the motion of Uranus, as between the calculations on the basis of the inverse square law and the actual observations of the planet. It was natural, therefore, that the discrepancy in the case of Mercury should also lead to a similar explanation, and immediately the search for the missing planet began. It was actually "seen" by a French amateur astronomer, and it was named Vulcan—but nobody else has ever seen it since, even with the greatest and most powerful of modern instruments! If a planet existed so near to the sun, large enough to affect appreciably the motion of the perihelion of Mercury, then it would be large enough to be easily visible, especially in transits when it passed between the earth and the sun. With great reluctance the planet Vulcan was relegated to the limbo of scientific might-have-beens; but the search for some other gravitating matter that had not been thought of yet, and that would account for the discrepancy in the motion of the perihelion of Mercury, continued. A ring of small planets surrounding the sun—like the tiny satellites that constitute Saturn's rings according to J. Clerk-Maxwell (1831-1879)—was tried; it was suggested that the Sun has a certain amount of ellipticity, so that the attraction produced by it is not exactly proportional to the inverse square of the distance from its centre; it was thought that the Sun's corona may attract sufficiently to explain Mercury; perhaps Mercury had a satellite—all these ideas led nowhere. It is true that an elaborate discussion of the zodiacal light has afforded a means of dealing with the outstanding discrepancy in Mercury, as well as with some less important discrepancies in the motions of the other inner planets, but it can hardly be disputed that somehow this theory leaves one unconvinced, and is a too obvious patch upon a view of gravitation that was originally heralded as the greatest and simplest generalisation in astronomical science.

It was in imitation of Laplace that the second type of explanation of the difficulty with Mercury arose. Laplace suggested that possibly space absorbs

gravitation just as glass or water absorbs light. A negative exponential function of the distance from an attracting particle, when introduced as a factor into the ordinary inverse square formula, can give an additional motion of the perihelion of Mercury—but it spoils all the other planets. If we take the law of gravitation to deviate a little from the inverse square of the distance, and make it an inverse power slightly different from two, we can again explain Mercury, but the moon is upset. Of course we can add on to the force proportionate to the inverse square of the distance another small force varying in some other way, *e.g.* the inverse cube or the inverse fourth power of the distance, and the perihelion of Mercury will be explained if we choose the value of this additional force correctly. But surely we are merely laying a gratuitous patch upon the beautiful law enunciated by Newton. Why should this patch be ? What natural, physical thing does it represent ?

We have already mentioned that Lorentz failed in his attempt to use an electrical theory of matter in order to explain gravitation in such a way as to deal with the perihelion of Mercury. As a matter of fact, this third type of attempt to deal with Mercury is also due ultimately to a suggestion of Laplace. In Newton's law of gravitation the question of the mechanism of the propagation of the gravitational force is not dealt with—indeed, the propagation of gravitation is not contemplated at all, and the effect at any point in space due to a particle of matter at any other point in space is simply given by the mass of the particle divided by the square of the distance at the instant considered : in other words, the law of gravitation is constructed on the basis of an infinite speed of propagation of gravitational effects !

It was Laplace who suggested the introduction of a finite speed of gravitational propagation, in the form of a sort of aberration effect. Unless this speed is taken to be vastly greater than even the velocity of light, serious effects are produced on the mean distances and consequently on the mean motions of the planets. But the electromagnetic developments of the nineteenth century made it clear that, if gravitation is propagated, then the velocity of propagation must be the same as the velocity of light—the velocity of light was gradually emerging as a fundamental quantity in the economy of nature. Other forms of the propagation effect, based upon the velocity of light, were therefore introduced, analogous to the forms suggested from time to time in electromagnetic theory. Thus Weber and C. G. Neumann (1832-) developed an expression for the electric force produced by a moving charge. F. F. Tisserand (1845-1896) calculated the motion of a planet acted upon by a gravitational force analogous to this electric force, and an additional motion of the perihelion of Mercury was obtained. But the amount thus obtained was about one-third of the amount required. G. F. B. Riemann (1826-1866) developed a different expression for the electric force, and this, when applied to gravitation, gave an additional motion of the perihelion of Mercury equal to two-thirds of the amount required. It was then suggested that if the actual force is taken to be twice the amount suggested by Riemann's formula *minus* the amount suggested by Weber and Neumann's formula, then the whole of the discrepancy in the perihelion of Mercury could be explained. Even more bizarre ideas were advanced, but they only indicated the hopelessness of the situation.

It is not the object of this address to deal in any detail with the theory of relativity as such : numerous accounts of this theory have been published, and it would be out of place to give here a brief outline of a theory which penetrates into the most fundamental aspects of our conception of nature. Suffice to say here that the optical developments of the nineteenth century, especially the epoch-making experiments of A. A. Michelson (1852-) and other investigators, led Lorentz to introduce a new conception of time—*Eigenzeit*—and to the formulation of the famous Lorentz transformation for coordinating observations with reference to frames of reference in motion relative to one

another. Lorentz conceived the idea of using Laplace's suggestion of a finite velocity of propagation of gravitation with the aid of his transformation. The attempt was partly successful in the sense that it became possible to endow gravitation with a finite velocity of propagation—in fact, the velocity of light—but the outstanding Mercury discrepancy remained. Later developments on the same basis made hardly any improvement, and the beginning of the twentieth century found the dual problem bequeathed by Newton two centuries before still unsolved: the motion of the perihelion of Mercury was different in fact from what it should have been by theory, and a satisfactory physical explanation of gravitation seemed to be as unobtainable as ever.

It has already been remarked that when mechanical models fail us we naturally tend to dispense with such models altogether. Universal gravitation as a physical force, explainable in terms of other and well-known physical forces, dominated the minds of men for three hundred years. The inherent tendency of all matter to congregate together in the form of spheres, enunciated by Copernicus in 1543, gave birth to a multiplicity of physical theories of gravitation, from the beginning of the seventeenth century to well into the twentieth century—and all these theories broke down. The inevitable consequence followed. It was Albert Einstein (1879-) who made a serious and successful attempt to deal with gravitation as a fundamental phenomenon of nature, independent of other physical phenomena, and existing in its own right. No mechanical picture in the traditional sense was therefore to be looked for in order to account for the phenomena attributed to gravitation. If the Greeks postulated that earthly matter *must* move in lines through the centre of the earth, and that celestial matter *must* move uniformly on circles round the earth, Einstein postulated that the heavenly bodies move in the way they do, because the properties of time and space are such that the motions actually observed are the motions naturally flowing from these properties, in other words because the heavenly bodies *must* move in the way they do. If in the space-time as understood by Galileo and Newton a particle left to itself moves uniformly in a straight line, Einstein's space-time is so postulated as to make a particle left to itself move in the manner given by the law of gravitation.

This statement, intentionally bald and blunt, does not do justice to the genius of Einstein and to the far-reaching character of his generalised theory of relativity. But this is a lecture on gravitation and not on relativity in its widest sense. The effect of a gravitational field upon the path of a ray of light, the shift in spectral lines, the beautiful summing up of the dynamics of the universe in one all-inclusive principle of the invariance of a world function, these and other consequences of the theory of relativity are beyond our province here. As far as the fundamental theme of gravitation is concerned, Einstein's theory is a mathematical or geometrical presentation of the effects of gravitation, expressed in terms of simple postulates and beautiful analysis—almost a mystical enunciation, as all purely mathematical enunciations, without masses like lumps of coal and forces like pokers, must appear to "the man in the street."

Einstein conceives a space-time continuum with three space coordinates x, y, z and one time coordinate t; not space and time existing in absolute independence of one another, but space-time in such combination that the only physical reality that we can conceive as having existence as an invariable entity is a general quadratic expression in terms of the differentials dx, dy, dz, dt, the coefficients of the squares and products of dx, dy, dz, dt being themselves functions of x, y, z, t. As a matter of fact, it is not a difficult matter to present classical dynamics on a similar basis. But whereas in classical Galileo-Newtonian dynamics the coefficients in this quadratic expression are very restricted in type, in Einstein's conception of space-time the basis of dynamics

is such that the coefficients are not, in general, restricted at all. Now in the space-time so defined consider the time coordinate as if it were a space co-ordinate. This idea, so beautifully developed by H. Minkowski (1864-1909), is often taken to mean that relativity actually considers time to be space (with a root-of-minus-one thrown in to make sense). It is hardly necessary to point out that this is a gratuitous addition to the theory, which does not contemplate any physical model at all. A four dimensional "space" is constructed merely so that we may apply to it certain algebraical and analytical operations, analogous to and extensions of corresponding well-known operations in ordinary two- or three-dimensional geometry. In particular, we take the notion of curvature as applied to a one-dimensional continuum, *i.e.* a curve in a plane, and to a two-dimensional continuum, *i.e.* a surface in Euclidean three-dimensional space, and having expressed it in appropriate analytical form, we construct the extended expression applicable to a four-dimensional continuum. We deduce a so-called tensor with, nominally, 256 components, but with, really, only 20 independent components. These are the "components of curvature" of the continuum. If they are all zero, the continuum is "flat," and the motion of a particle in such space is rectilinear and uniform. In general, they are not all zero, and the paths of particles, namely geodesics in this four-dimensional continuum, defined analogously to ordinary geodesics on surfaces in Euclidean three-dimensional space, are no longer straight and uniformly described.

A gravitational field is defined as a space-time continuum in which the components of curvature, while not all zero, nevertheless satisfy a certain number of equations in which the components of curvature occur linearly. Nominally there are 16 such equations for the 256 components: really there are 10 equations for the 20 independent components. By applying these equations to a space-time continuum in which the space is supposed to be symmetrical about a point, we get the gravitational field due to a "point mass," or rather, indicating the existence of such a mass (or an equivalent sphere with its centre at the point of symmetry).

Now come the remarkable features of the gravitational aspects of the theory of relativity. In the first place a theory of gravitation is produced—not a physical theory with a mechanical model, but a mathematical, algebraic-geometric theory. The gravitational field produced is extraordinarily like that enunciated by Newton: to all intents and purposes the inverse square law is produced by the relativity theory of gravitation. All the difficulties experienced in the numerous older attempts at a theory of gravitation disappear as if by magic.

But there is a slight discrepancy between the law of gravitation as given by Newton and the law of gravitation as deduced by Einstein—and this discrepancy turns out to be one of the major triumphs of the theory. The difference between the inverse square law enunciated by Newton and the more complicated law of gravitation deduced by Einstein is just what is required, in order to account for the discrepancy between the motion of the perihelion of Mercury as calculated in accordance with the inverse square law and as observed in the heavens.

The significance of the Einstein method of reconciling Mercury with theory is not always appreciated, and one still finds attempts being made to deny the necessity for the relativity theory of gravitation by showing that it is possible to postulate some *ad hoc* theory, some additional term in Newton's law, so as to account for the perihelion of Mercury discrepancy. It must be understood that Einstein's theory does two things (in addition to many others): it gives a theory of gravitation, and it gives a reconciliation between theory and observation of Mercury, the latter being not an *ad hoc* assumption or postulate, but a natural and necessary consequence of the theory of gravitation itself. There is even no assumption made as to the value of some constant,

as is usually the case in an *ad hoc* theory. The motion as observed agrees with the theory, and could not be otherwise than it is proved to be.

We have reached, therefore, an interesting situation in the development of the subject of gravitation. Newton's two legacies have now been duly assimilated into the body of science. As things stand to-day, there are no very large outstanding differences between theory and observation in connection with the motions of the solar system. Further, a mathematical, or geometrical, theory of gravitation itself has been deduced. Physical gravitation has practically disappeared from the minds of men. Gravitation has become a property of the very substratum of all experience, namely the space-time in which all our experiences take place, to which all events are referred. Gravitation is a property *per se*, it exists in its own right, it is independent of all other physical states or conditions.

Is this the end? Surely we may assume that we have not reached the final step in the evolution of our views of gravitation. Our scientific experience speaks to us too loudly and too insistently of the mistakes of our forerunners, who thought they had reached finality, when they had in reality hardly penetrated even skin-deep into the subject in hand. We are clearly passing through a mathematical-geometrical phase of gravitational concepts. It is not unsafe to predict that the mind of man will react to this phase as it has reacted to similar phases before now. It will not rest content with a theory of gravitation which is at bottom a synthesis of algebraical analysis, geometrical philosophy and logical plausibility. The remarkable achievement of Einstein must lead to a new "materialistic" picture of gravitation, to a fresh "mechanistic" theory, which will make gravitation respond to the desire of man to grasp realities in a manner that appeals to his sensual nature.

There seems to be little in existence to-day that suggests such a development. In a sense we can say that such a mechanical picture may be quite beyond our present-day conceptions of things. Just as the Greek view of gravitation, with its earthly rectilinear and its celestial circular motions, gave way to the physical view of gravitation of Newton's theory only through the pangs of a very fundamental modification of our conceptions of matter and motion, and Newton's theory gave way to the geometrical view of Einstein's theory only through the pangs of a very fundamental modification of our conceptions of time, space and matter, so we may perhaps anticipate that the next step in this process, the emergence of the new physical theory of gravitation, will only take place after very fundamental changes in our conceptions of the substrata of natural phenomena. Einstein's theory has ended a chapter in science, but it has also begun a new chapter, which our successors will be privileged to read.

S. BRODETSKY.

GLEANINGS FAR AND NEAR.

561. If A be equal to B, there must not only be reason, but reason enough for it; anything short of reason enough is no reason at all, and anything short of proof enough is no proof at all.—De Morgan, P.C. 23. p. 209.

562. In 1782 Condorcet was elected into the Academy. His competitor was Bailly, over whom he had a majority of one. The true contest lay less between the two candidates than between d'Alembert and Buffon, who on this occasion are said to have fought one of the greatest battles in the not peaceful history of the Academy, for mighty anger burns even in celestial minds. D'Alembert is said to have exclaimed, we may hope with some exaggeration, that he was better pleased at winning that victory than he would have been to find out the squaring of the circle.—Morley, *Misc.* Vol. ii. p. 185.

563. The story of Wolfe and Gray's *Elegy* is confirmed by Professor Robison, of Edinburgh, who began as middy and was in the boat with Wolfe.

SOUND-RANGING.*

By W. Hope-Jones, B.A.

"We are all pacifists now"; or if not, it is difficult to see what excuse we have for living in the modern world. I feel therefore that a word of apology is needed for a paper on a military subject. But I hope you will acquit me of any intention to revive the hatred and bloodthirsty panic that poisoned the years 1914 to 1918, because the activity that I have to tell you about is not concerned with the destruction of life but rather with the saving of it.

The location of invisible guns by means of sound was first practised by the French in 1915. Sound-ranging in the British army dates from October 1915, when W. L. Bragg, after a course of training with the French, began work at Kemmel Hill. Under Bragg's guidance the British sound-rangers reached a state of efficiency which eventually surpassed both the French and the Germans.

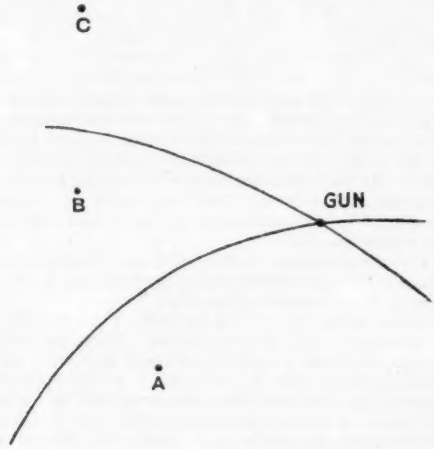


Fig. 1.

Take A and B to be carefully surveyed positions in which microphones have been placed electrically connected with headquarters. G is an enemy gun: when it fires, the sound of its report spreads in every direction and reaches first A and then B. If each of these microphones tells us when it hears the report, the interval between these two times gives us $GB - GA$. For instance, if B hears the report a second after A, $GB - GA = 337{\cdot}6$ metres, the distance travelled by sound in one second. The locus of all points for which this is true is a half-hyperbola with A and B for foci. Now if we have a third microphone C, we are able to find also $GC - GB$, which fixes our gun on to another half-hyperbola, with foci B and C. The intersection of these two curves should give the actual position of the gun.

For greater realiability we used six microphones, arranged generally in an arc of a circle of 35 seconds radius, each one at a distance of $4\frac{1}{2}$ seconds from its neighbours. The intersection of the five hyperbolas, when it came off satisfactorily, verified the position of the gun beyond any doubt.

* An Address at the Annual Meeting of the Mathematical Association, Jan. 6th, 1928.

Fabulous stories were current among the later British sound-rangers about the gigantic size and complication of the earliest microphones used by the French when first they used this method of locating guns. These monsters were no longer in use when I became a sound-ranger in 1917, but legend by that time had invested them with a weight so huge that two men were needed to carry one. A year later we were teaching newcomers that the old French

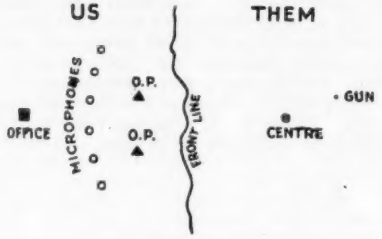


FIG. 2.

microphones of the early days of sound-ranging required a lorry each to carry them, and that as often as not the lorry broke down in the attempt to deliver the goods at the proper microphone-station, which might be in very rough ground far from any road. These microphones became extinct for very much the same reason as the Brontosaurus, who was too big to compete with his tiny intelligent rivals, such as man. And man in this case is represented by the Tucker microphone, a little instrument so handy that I have often carried six of them in my tunic pocket.*

This is what the official *Report on Survey on the Western Front* says of the Tucker microphone: "From the date of its introduction Sound-ranging may be said to rank as an exact method of location."

Here is the Tucker legend for you: I have Mr. Tucker's own authority for assuring you that every word of it is untrue. Early in 1916 Tucker was sleeping in an Armstrong hut, a form of residence which has all the comforts of the modern home except that if you touch it a hole appears in the wall. And it so happened that this particular Armstrong hut had a hole in it very near to Tucker's face. A German aeroplane came over, which did no harm; but our anti-aircraft guns opened fire at it, which did. For every time a gun

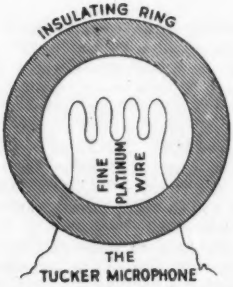


FIG. 3.

* Mr. W. S. Tucker kindly lent a microphone for exhibition at the meeting.

went off, with that peculiarly sharp "Pank" that "Archies" always make, the front of a sound-wave arrived at the hole in Tucker's Armstrong hut and blew a vicious puff of cold air through the hole slap on to Tucker's face, interfering sadly with his slumbers, but stimulating his brain to most profitable activity. Wherefore Tucker took unto himself a tin can * and made a hole † in it. Into this hole he stuffed a ring of some insulating material with a fine platinum wire running across the ring; and through the platinum wire he shoved an electric current ‡ just strong enough to keep it red-hot. This current remained constant until somebody fired off a gun: when the arrival of the front of the sound-wave compressed the air just outside the hole, it shoved a puff of air through it, just as it had through the hole in the Armstrong hut; but this time it was the platinum wire instead of Tucker's face that took the cold puff. Now it seems that the electrical resistance of platinum wire decreases when you cool it; so without the intervention of any moving parts our German friends were persuaded to alter the resistance in an electric circuit which was in our keeping whenever they let off a gun. And that insulating ring and platinum wire are the Tucker microphone.

Through our playing-fields at Eton there run two little tributaries of the Thames, Jordan and Willowbrook. They are connected by a cross-ditch in which the water flows sometimes in this direction, sometimes in that, according to the activity of the two streams. Sometimes the water in this cross-ditch stands still, when the Jordan end and the Willowbrook end of it are at the same level. If at such a time you throw an obstruction into Jordan, you set the water running along the cross-ditch towards the easier Willowbrook outlet into the river; if, on the other hand, you clean and dredge the Jordan

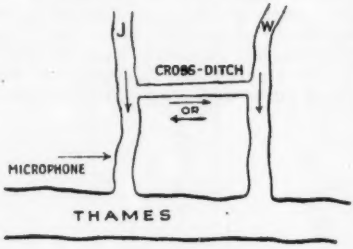


FIG. 4.

channel you divert water into it which comes bustling in from Willowbrook. When electricians treat their electric currents like that, they call it a Wheatstone Bridge. Every microphone was connected with a Wheatstone Bridge in our office, some two miles behind the microphone base, where most of our apparatus was kept. The Tucker microphone was in this Jordan wire; and as soon as the arrival of a sound-wave decreased the resistance in the microphone wire, which you can compare to dredging out the Jordan bed, a current started running through this cross-ditch wire, which was in the very middle of our apparatus under the closest observation.

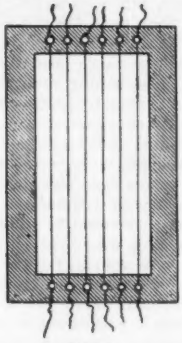
Every one of the six microphones was connected like this to a separate Wheatstone Bridge of its own; and the six cross-ditch wires were arranged side by side in a little frame called the "harp," which we kept between the poles of a powerful electro-magnet. When a current flows through a wire in

* He says that the first tin-can was a wooden map-box.

† He says that Sergeant Clark bored the first hole.

‡ He says that he never shoved the current: it shoved itself.

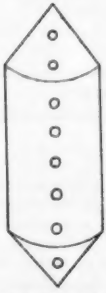
a magnetic field, the wire sags out sideways for some reason which no doubt you could explain to me much better than I could explain it to you. Notice that this little wire, a good deal thinner than a human hair, is the first physical



HARP

FIG. 5.

object that has been moved in the recording of this sound; the rest has all been done by variation of temperature, resistance and current. So the German fires his gun, the six microphones all hear it in turn, and as each microphone hears it fire, that microphone's own particular wire in the harp bellies out sideways until the wire in the microphone has warmed up again, which doesn't often take longer than a tenth of a second. Tucker's tin cans were made with a few extra holes in them for the sake of damping the oscillations of the air. Without these, waves of air would have gone on sweeping in and out of the



CONTAINER FOR TUCKER MICROPHONE

FIG. 6.

microphone hole for a second or more, cooling the microphone and keeping the harp-wire bent, whereas we wanted it back again in its normal state as soon as possible, ready to record a second report if it should come soon after the first. Also they were made in this kind of shape (Fig. 6) because it looks so nice and scientific; and we called them "containers" instead of tin cans because it sounds more professional.

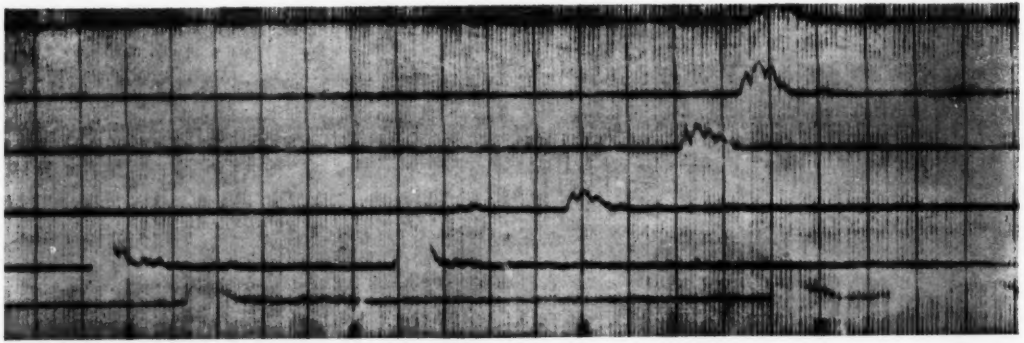


FIG. 7, SLIDE I.

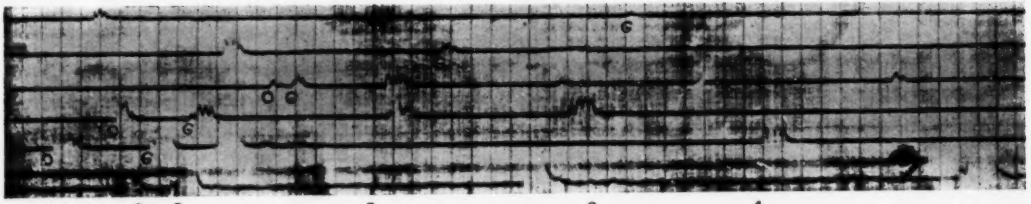


FIG. 9, SLIDE II.

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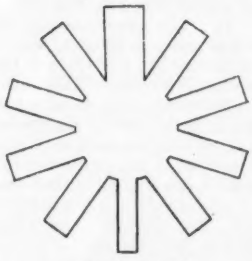
You must imagine yourself now sitting in the office behind the microphone-base. The enemy gun fires, and all your little harp-wires in turn strain out into a curve as each microphone hears the report. Evidently the next thing needed is a graphical way of recording the intervals between the movements of the six harp-wires.

For this you have a lamp shining through a slit and casting a thin slab of light in a plane at right angles to the wires of the harp, so that each wire casts a point-shadow, being the shadow of the little element of the wire which is in the plane of the slit and the lamp. Reflecting prisms catch this light and throw it, with its six point-shadows, on to a strip of cinematograph film * (Slide 1), which is moved along at about one inch per second. This passes through an automatic developer and fixer so quickly that when our office was further behind the line than usual, and the enemy was firing at it, I have even had the film in my hands before the shell arrived. In fact, there was in my section an officer with a remarkable visual memory who could often recognise a gun which he had located before by a glance at its film. I have even known him to report the gun by telephone before the shell whistled over our roof.

As long as a microphone hears no noise, its harp-wire remains still and throws a stationary point-shadow on the film, while the steady movement of the film causes this point to trace out a straight line along it. But as soon as a microphone hears a gun fire, its harp-wire starts sideways, its point-shadow moves at right angles to the length of the film, and the straight line is broken. These breaks are the data from which we have to deduce the position of the gun.

In front of the lamp a little wheel called the time-wheel was going round, controlled by a vibrating tuning-fork which made and broke a current at every vibration. This wheel had ten arms, one being bigger and the opposite one



TIME-WHEEL

FIG. 8.

smaller than the rest: it made ten revolutions a second. The result of this was that 100 times a second the light was shut off from reaching the film for one or two thousandths of a second, so that a dark line was made across it: a stronger dark line every tenth helped with the counting, and so did the weaker lines half-way between the strong ones. These lines graduated our film for us into hundredths of a second, corresponding to 3·376 metres, or about 11 feet. This was the unit of time and distance which we always used.

The sound from this gun reached number 6 microphone first, number 5 46 hundredths of a second later, then 4 after a further interval of 38 hundredths of a second, represented by the spaces between these vertical lines on the film:

* Mr. J. H. Hope kindly lent four lantern-slides.

the later intervals are 23½, 13½, and 2 hundredth-of-a-second units. The actual position of the gun is marked on Fig. 2, the microphones being numbered from the south.

The time-wheel and the tuning-fork which controlled it were kept going continuously, because starting them required some care and couldn't be done automatically by switching on a current. But the rest of the apparatus, with its various currents for the six microphone-circuits, the lamp, the electro-magnet, and the motor which draws the film along and feeds it into the automatic developer, was working quite happily within two seconds of the current being switched on. In front of the microphone-base we had two observation-posts, where observers listened for the sound of enemy guns firing. On hearing one they immediately pressed a button which completed the circuit and started the rest of the instrument going. By the time the sound reached the microphones the platinum wires were red-hot, the electro-magnet was fielding, the lamp was shining, and the motor was turning out about an inch of film per second past the point of illumination and into the developer.

I have given you only the very roughest outline of the wonderfully delicate and beautiful apparatus which recorded in our office the experiences of the Tucker microphones away in the open country. It was made by Lucien Bull, a British subject resident in Paris; and I believe it was the experience of all of us that the more we used it the higher rose our admiration of this man's genius.

This slide illustrates one of the principal difficulties of Sound-ranging: one may call it *embarras de richesse*. In noisy times it was often a troublesome business to pick out from among the variety of stray sounds those which were caused by the gun we were looking for. I am told there are records of two guns on this film, but I have only been able to find one. However, I have known some sound-rangers who would have no difficulty in discovering six on it if they were feeling in a genial and optimistic frame of mind. The breaks which I have marked with a G at 330, 235, 158, 106, 85, and 96 give intervals of 95, 77, 52, 21, and -11: the corresponding hyperbolas intersect very cleanly at a point about five miles in front of the microphone-base, a little north of the middle, because number 1 is the southern microphone, which is evidently the last for the sound to reach. That is one reason why the sound is so faint on number 1: also there seems to have been a south wind blowing which makes the sound reach the southern microphones rather feebly. Notice also these breaks marked O, of which I shall have some more to say later on: the breaks which I have marked O are characteristic of a fast bowler, a gun which sends off its shell with a speed greater than that of sound.

The early sound-rangers actually drew hyperbolas on their maps and located guns by their intersections. But the labour involved was enormous, and the locations took far too long to be valuable. Guns are nearly always so far from the microphone-base that the asymptote is a very good approximation to the part of the hyperbola near the gun. This fact is used to give a method of deducing the gun's position from the intervals read from the film with surprising speed and accuracy.

A and B are two consecutive microphones at a distance a apart. G is a gun at a distance r from X, the middle point of AB, in a direction making an angle θ with AB. Then we can read from our sound-ranging film the interval $GA - GB$, which we will call c. Then

$$GA, \text{ that is } \sqrt{r^2 + ar\cos\theta + \frac{a^2}{4}} - GB, \text{ that is } \sqrt{r^2 - ar\cos\theta + \frac{a^2}{4}} = c.$$

We will solve this as an equation to find $\cos\theta$ from;

$$\therefore \sqrt{r^2 + ar\cos\theta + \frac{a^2}{4}} = \sqrt{r^2 - ar\cos\theta + \frac{a^2}{4}} + c;$$

$\therefore$ squaring both sides,

$$r^2+ar\cos\theta+\frac{a^2}{4}=r^2-ar\cos\theta+\frac{a^2}{4}+2c\sqrt{r^2-ar\cos\theta+\frac{a^2}{4}}+c^2;$$

$$\therefore\ 2ar\cos\theta-c^2=2c\sqrt{r^2-ar\cos\theta+\frac{a^2}{4}}.$$

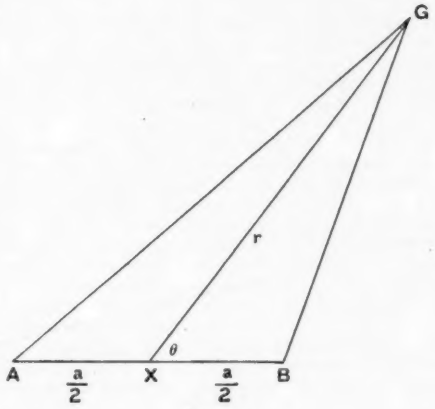


FIG. 10.

Again square both sides;

$$\therefore\ 4a^2r^2\cos^2\theta-4arc^2\cos\theta+c^4=4c^2r^2-4arc^2\cos\theta+a^2c^2;$$

$$\therefore\ 4a^2r^2\cos^2\theta=c^2(4r^2+a^2-c^2);$$

$$\therefore\ a^2\cos^2\theta=c^2\left(1+\frac{a^2-c^2}{4r^2}\right);$$

$$\therefore\ a\cos\theta=c\sqrt{1+\frac{a^2-c^2}{4r^2}}.$$

r is in most cases much bigger than a and c: so $a\cos\theta=c$ is a useful first approximation. a is the distance from one microphone to the next, generally 450: c is the interval read from the film, and hence θ is immediately deduced from c.

Fig. 11 represents a partly-made plotting-board: it lies horizontally on a table. At X there is a hole through which passes a fine gut line with a small weight on the end, hanging below the board: at the other end of the line is a heavier weight looking rather like a mouse, with enough friction to stay where you put it on the plotting-board. This board itself is a map, gridded in the same thousand-yard squares as the ordinary military maps by which all references were given. The line from X to the gun is to make an angle with AB given by the equation $a\cos\theta=c$. A band round the edge of the board is graduated in values of c, from -450 to plus 450, so that if the string is laid down over any value of c it makes an angle θ with AB such that $a\cos\theta=c$. Then if we neglect for the time the difference between c and $c\sqrt{1+\frac{a^2-c^2}{4r^2}}$, this string passes through the position of the gun.

The complete plotting-board (Fig. 12) has five scales, five mice, and five gut-lines, corresponding to the five sub-bases, AB, BC, CD, DE and EF, which

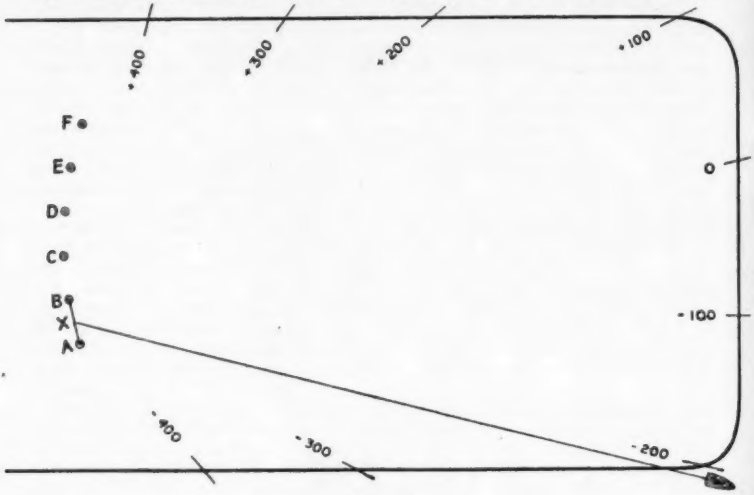


FIG. 11.

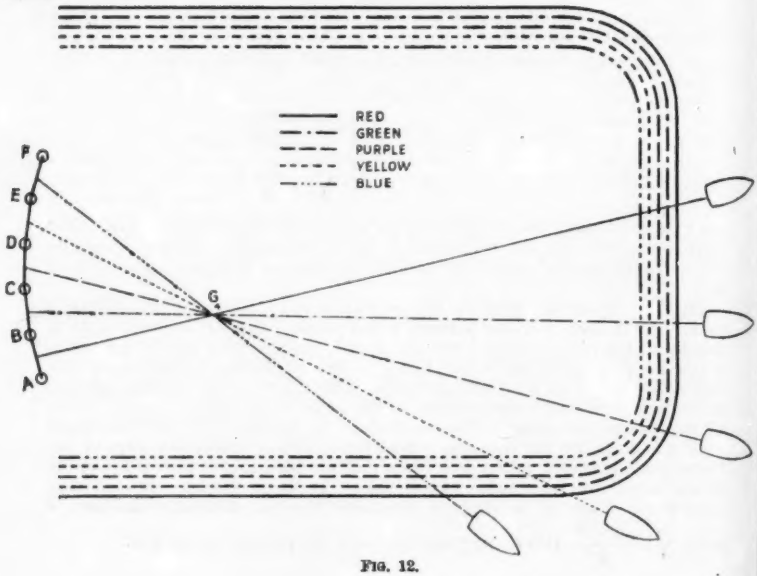


FIG. 12.

are coloured so as to show which corresponds to which. I am sorry I have forgotten just which colours we used for which ; but I expect there are enough old sound-rangers here to tell me that I have coloured them all wrong.

The approximation to the position of the gun got by this intersection is often so good that there is no need to apply the correction for the difference between the actual hyperbola and this string which is its asymptote. $a \cos \theta$, which should really be $c\sqrt{1+\frac{a^2-c^2}{4r^2}}$, has been taken as c in getting this first approximation. The difference between these two is called the "Asymptote-correction," and is tabulated in advance for likely values of r and c. If the first approximation is not thought accurate enough without it, the asymptote correction for each string can be looked up as soon as r can be measured approximately, and a small movement of the strings can be made so as to bring their intersection on to the actual gun-position.

The temperature-correction is a very mild affair. The velocity of sound in air is 337·6 metres per second at some standard temperature : it increases by one-thousandth for every increase of 1 degree Fahrenheit. Every interval measured from the film has therefore to be corrected by a fraction of itself depending on the temperature. Notice also that if the tuning-fork controlling the time-wheel is not vibrating at exactly the right rate, the correction to be applied for that is of the same sort, a constant fraction of every interval as measured from the vertical lines on the film. So the temperature-correction and tuning-fork-rate-correction can be lumped together into one : if your tuning-fork vibrates too fast by one per cent., all you need to do is to take your standard temperature as ten degrees hotter than a neighbouring section whose tuning-fork vibrates at its proper frequency.

The wind-correction is the most interesting of these corrections.

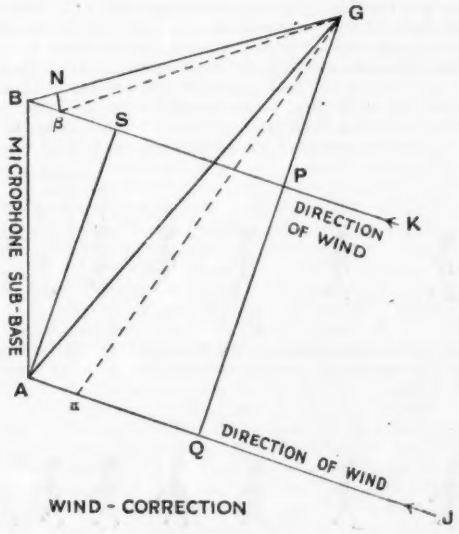


FIG. 13.

A and B are two consecutive microphones. AB is one of the five sub-bases of which the whole microphone-base is made up. JA and KB are drawn in the direction of the wind, which is blowing with a speed equal to a fraction v of the speed of sound. v is a "small quantity," and the ordinary approximations implied in that name are used. AS and GPQ are at right angles to the direction of the wind.

α is a point on JA such that $A\alpha = v \,.\, G\alpha$: so the sound travels from G to A by doing the component $G\alpha$ by its own efforts and being carried the component αA by the wind. Also sound gets from the gun to B by travelling $G\beta$ relative to the air and being carried βB by the wind.

Our sound-ranging film gives us $G\alpha - G\beta$: what we want is $GA - GB$: so the wind-correction is

$$(GA - G\alpha) - (GB - G\beta).$$

Using the obvious approximations, $GB - G\beta = BN$, βN being at right angles to GB. $BN = B\beta \cos GBK$, that is $v \,.\, GB \cos GBK$. For $GB \cos GBK$ write BP. Then $GB - G\beta = v \,.\, BP$. In the same way $GA - G\alpha = v \,.\, AQ$. Therefore the correction required $= v\,(AQ - BP) = -v \,.\, BS$.

Thus we arrive at the result that the wind-correction is independent of the position of the gun, and is equal to the projection of the sub-base on the direction of the wind, multiplied by the fraction $\dfrac{\text{speed of wind}}{\text{speed of sound}}$, and given its appropriate plus or minus sign. This is a very easy quantity to calculate in advance and tabulate.

If you stand half a mile in front of a high-velocity gun, which discharges a shell past your head with a speed greater than that of sound, the first thing you hear is a violent crack which you mistake at first for the firing of the gun. Next you hear the shell whistling along towards you and at the same time from the opposite direction you hear it whistling on away from you. Last of all you hear a duller thump, which is actually the firing of the gun, but if you have mistaken the first crack for that, you will take this for an echo. This first loud crack is due to a pressure-wave set up by the shell itself in its motion through the air: it caused a great deal of difficulty to the earlier French sound-rangers who mistook it for the gun at first. Also the microphones of the pre-Tucker age had generally not recovered their composure after hearing this first noise in time to record the firing of the gun when the sound from that arrived a little later. We called this bang the "Onde-du-choc": I want now to outline a little of the simplest part of the theory of it.

FIG. 14.

Imagine a bus going along a road with decreasing speed, faster at first than you can walk, but gradually slowing to less than walking speed. Imagine

FIG. 15.

passengers perpetually getting off it and pursuing their journey on foot along the same road. Then at first the passengers string out behind the bus, which goes ahead of them like this. Those who get off early are strung out further apart than those who get off when the bus is going only a little faster than they walk. When the bus is going at a walking pace, the passengers who get off walk along by the side of it in a solid mass which keeps on receiving reinforcements from new disembarkers. As the bus slows down still more,

FIG. 16.

the passengers who have already got off begin to go ahead of it, and the later walkers are mixed up with the earlier and strung out. The crowd is headed by a dense mass of all those who got off when the bus was going with a velocity close to that of a walker.

Now imagine yourself awaiting this crowd at the terminus. Suppose it is a public-house and you are the barmaid. What will you observe? The vanguard of this army will crash through your door almost simultaneously and demand their respective beverages with a multiplied intensity resembling the force of an explosion. Notice that the distribution of this crowd is exactly opposite to that of the oysters of whom we are told that "thick and fast they came at last, and more and more and more." It is at first that these travellers arrive thick and fast, thinning out afterwards to less and less and less.

This bus is the shell, travelling with a speed greater at first but afterwards slower than the walkers, who are the sound. As the shell travels, it sets up sounds, or air-waves, which behave just like these walkers. Those which are given off by the shell when it is travelling just about as fast as sound reinforce each other and arrive together with a bang that sounds like an explosion. And that is the *onde-du-choc*.

If the destination which the passengers are making for is not on the bus-route, then for the actual speed of the bus we must substitute the component of that velocity towards the destination. Fig. 17 represents the wave-front of the walkers who have left the tram approaching their destination, the leaders jostling each other shoulder to shoulder. The human animal is a difficult one to draw in plan, and unfortunately I was unable to obtain an aeroplane-photograph of the scene. The first walkers to arrive are those who get off when that component is equal to their own walking speed. So any observer hears the *onde-du-choc* come from the place in which the shell was when the component of its velocity directed towards himself was equal to that of sound. If he is far out to the side of the gun, or behind it, he never hears the *onde-du-choc* at all. Places where it can be heard are all contained in a cone, the axis of which is the line of fire of the gun: the cosine of its semi-vertical angle is the speed of sound divided by the initial speed of the shell. The greatest interval between the hearing of the *onde-du-choc* and the hearing of the gun's report is on the axis of the cone: the interval decreases towards the surface of the cone; and on the surface the two sounds arrive together, giving the effect of an unusually loud gun-report. Every particular kind of gun had its own maximum *onde-du-choc* interval: it was often possible by this to find out not only where the gun was but also what kind of gun.

The breaks which I have marked with an *O* here are the *onde-du-choc*. This gun was firing in a direction between 4 and 5 microphones, rather nearer to 5: you can tell that by the intervals between the *onde-du-choc* and gun-report.

This gun is a much faster bowler than the last. The breaks which I have marked with *G* are the gun: the *O*'s are the *onde-du-choc*. We know that he is a faster bowler than the last, because the greatest interval between *onde-du-*

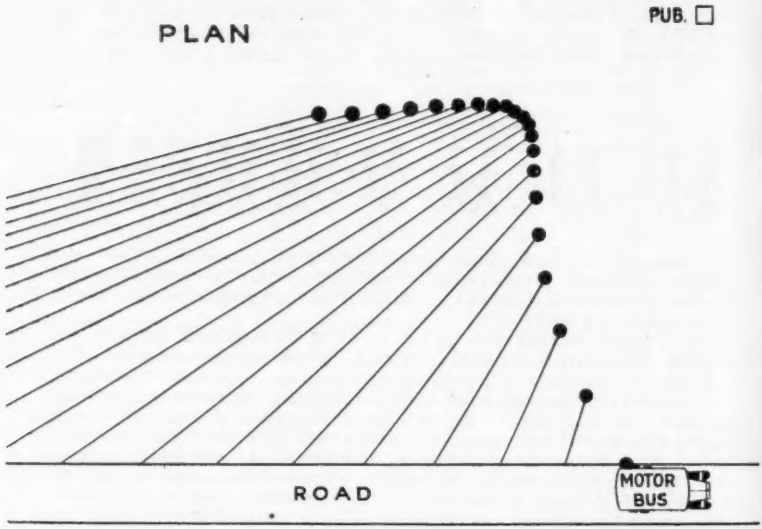


FIG. 17.

choc and gun-report is $1\frac{2}{3}$ seconds. Number 6 microphone is doing nothing here: either it was out of action or there was a north wind which it didn't like. Microphones are very human in a way: or perhaps a microphone would say that people are very microphonic.

This film was originally one continuous band; but it had to be cut into three bits in order to make a lantern-slide of it. It goes on for nearly 14 seconds, and is stuffed with history. Here is the gun, *G*, which you are going to see actually plotted. Here is the *onde-du-choc O*: the length of the intervals denotes that the gun is firing in a direction between microphones 3 and 4. Finally notice the breaks which I have marked with *B*. They come from something very much nearer to the central microphones than to the outer ones. The only position answering to this description is near to the middle of the microphone-arc itself, on the British side of the line; and this noise is, in fact, the burst of the shell thrown by that gun.

(Fig. 12 shows the gun on Fig. 19 plotted. The intervals are: $-10\frac{1}{2}$, $-63\frac{1}{2}$, -108, -147, -173.)

Notice now what happens if the *onde-du-choc* breaks are mistaken for the gun-report and plotted as such (Fig. 20). The strings outline an envelope which reminds you of a caustic by reflection. Now the wave-front of the *onde-du-choc* is a surface separating places which have not yet heard the shell moving through the air from places which have. This surface moves outwards steadily; and therefore I suppose its successive positions have all the same three-dimensional evolute. I suspect this caustic of being the intersection of that three-dimensional evolute with the ground; but I only throw that out as a suggestion for somebody who knows more about three-dimensional

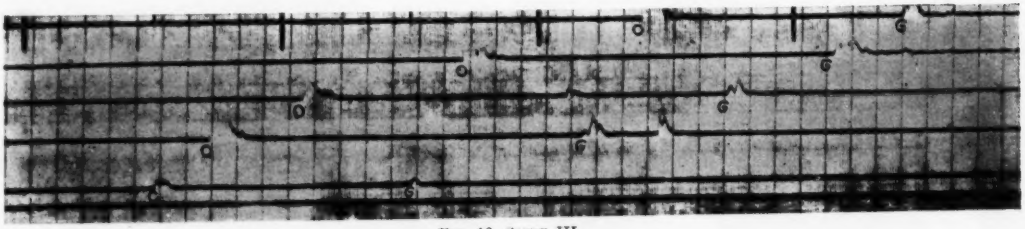


FIG. 18, SLIDE III.

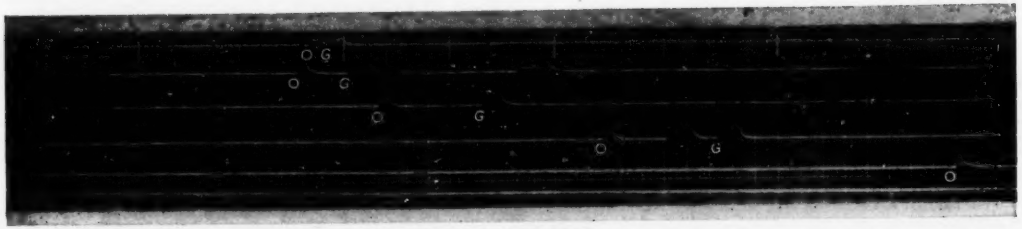


FIG. 19, SLIDE IV A.

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FIG. 19, SLIDE IV B.

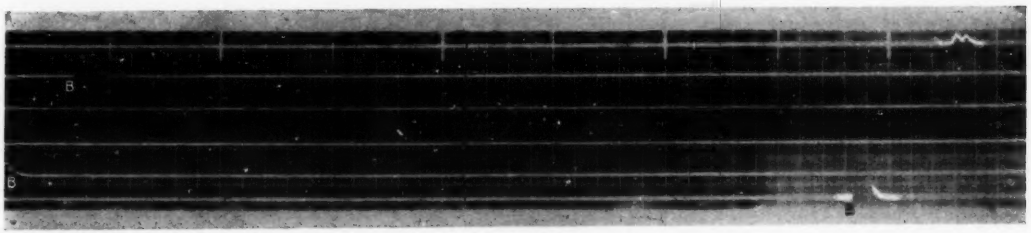
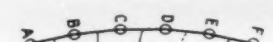


FIG. 19, SLIDE IV C.



evolutes than I do: he could hardly know less. The cusp of this caustic points in the direction in which the gun is firing, between microphones 3 and 4; it serves to give a line on to the gun even when unfavourable wind makes the gun-report inaudible.

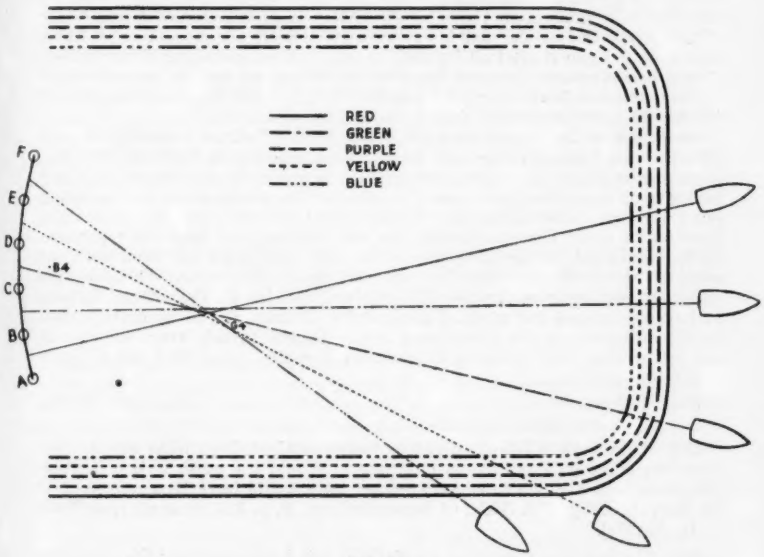


FIG. 20.

That leaves the shell-burst still to be plotted from the intervals 395, 346, 24, −331, −388. *B*4 on Fig. 20 shows it at the point for which the shell was heading when last we heard of it from its *onde-du-choc*.

Last of all we can find the time of flight. Choose one of the microphones (*M* on Fig. 21). *GM* and *BM* can be measured from the plotting-board when

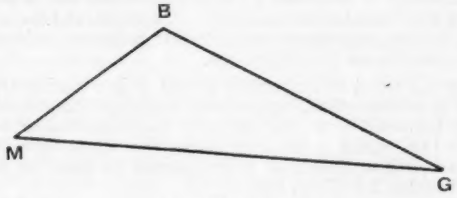


FIG. 21.

the gun and burst have been located. Now imagine a race from *G* to *M*. The winner is the sound, who goes direct. His time is *GM* (taking the speed of sound as unit speed); and this is known. The loser travels by shell from *G* to *B*: his time for this part of the journey is the time of flight which we want to find: call it *t*. From *B* to *M* he travels by sound, taking a time *BM*,

which is known. He loses the race by a time Q, which can be measured direct off the film. Therefore $t+BM=GM+Q$. Therefore $t=GM+Q-BM$.

If we take number 3 microphone, GM is measured as 1894, BM as 406, and number 3 heard the gun 422 hundredths of a second before it heard the burst: so $Q=422$. Therefore $t=1910$: so the time of flight is 19·1 seconds. The five other microphones give results all within $\frac{1}{10}$ of a second of this. The distance travelled by the shell is 16·2 seconds' journey for sound: so we know now that the gun shoved off its shell so fast that at one stage of its journey it was half a second ahead of the noise of the report, but the sound caught up the shell and beat it by 2·9 seconds. In fact, there is very little privacy left for the poor gun by the time we have finished with it.

And when we had found the guns, what then? Perhaps you might expect that we would immediately shell them. That would have been the best way of killing as many Germans as we could. But even in war there are better things to do than killing Germans: we used our knowledge more for protecting our own men. Just before our infantry went over the top, we turned the hose on to every German battery we had located, and kept their gunners underground out of harm's way—and a very good place for them too: so none of their batteries could open fire, and nobody got hurt. When the last great advance began on the 4th of November, 1918, the Flash-spotters, Airmen and Sound-rangers had marked down every German gun so accurately that on that occasion on the whole front of the Fourth British Army not one of our infantrymen was killed by German artillery fire. And that was a result worth working for.

Eton College. W. HOPE-JONES.

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564. If the rays of light had not moved in straight lines, what would have been the effect on our notions of figure? Would the straight line in that case have been the one to which all other lines would have been referred?—De Morgan, *Ency. Met.* (Calc. of Functions), vol. 2, p. 313, footnote [per Prof. E. H. Neville].

565. The beginner must bear carefully in mind that one quantity does not vary *as* another, because it varies *with* that other. A square and its root vary together, but the square does not vary *as* its root: if, for instance, the root be doubled, the square is not doubled, but quadrupled.—De Morgan, *Variation*, P.C. 26, p. 137.

566. We remember a book of arithmetic in which it was gravely asked, by way of exercise for the student, "If 6 had been the third part of 12, what would the quarter of 18 have been?" a question which can be paralleled only by "If a thing were both to exist and not to exist at one and the same moment, how many other non-existences would therefore become existences?"—De Morgan, *Sufficient Reason*, P.C. 23, p. 209.

567. Among the worst of barbarisms is that of introducing symbols which are quite new in mathematical, but perfectly understood in common language. Writers have borrowed from the Germans the abbreviation $n!$ to signify $1 \cdot 2 \cdot 3 \ldots (n-1)\, n$, which gives their pages the appearance of expressing surprise and admiration that 2, 3, 4, etc., should be found in mathematical results.—De Morgan, P.C. 23, p. 443

568. Of the same nature is the assumption which was till lately part of most proofs of Taylor's theorem, namely, that $\phi(x+h)$ can always be expanded in integer powers of h: and the consequence was that most works on the differential calculus were defaced by a subsequent admission that a proposition previously declared universal was not universal; and students received a practical caution to append "errors excepted" to every Q.E.D.—De Morgan, *Synthesis*, P.C. 23, p. 455.

THE REDUCTION OF A BILINEAR FUNCTION OF DISTANCES FROM FIXED POINTS TO A VARIABLE LINE.

By T. McHugh, B.A., and Prof. E. H. Neville, M.A.

Lemma. *If l is a line through the common vertex O of two triangles OPX, OYQ, then $P_l Q_l - X_l Y_l$ is independent of the direction of l if the two triangles are directly similar.*

§ 1. If O, P, Q, X are given, the condition of similarity determines Y uniquely. If we are given n pairs of points $P^{(1)}, Q^{(1)}$; $P^{(2)}, Q^{(2)}$; ..., where for particular values of r the points $P^{(r)}, Q^{(r)}$ may coincide, we can take an arbitrary base-point X and associate with the pair of points $P^{(r)}, Q^{(r)}$ the point $Y^{(r)}$ which is such that $OP^{(r)}X$, $OY^{(r)}Q^{(r)}$ are similar. Then for all values of r, $P_l^{(r)}Q_l^{(r)} - X_l Y_l^{(r)}$ is independent of the direction of l, and therefore if $a_1, a_2, \ldots$ are any n constants, positive or negative,

$$\Sigma a_r P_l^{(r)} Q_l^{(r)} - X_l \Sigma a_r Y_l^{(r)}$$

is independent of the direction of l. But if k is any constant other than zero, and if Y is the point such that the vector kOY is the sum of the vectors $a_1 OY^{(1)}$, $a_2 OY^{(2)}$, ... then $kY_l = \Sigma a_r Y_l^{(r)}$ identically for every line through O, and therefore

$$\Sigma a_r P_l^{(r)} Q_l^{(r)} - kX_l Y_l$$

is independent of the direction of l.

Not only does the construction determine Y uniquely, but it is easily shown that for a given position of X there can not be a second point Y' with the same property. For if Y and Y' have the same property, $kX_l(Y'_l - Y_l)$ is independent of the direction of l, and is therefore zero, since it is zero when l is the line OX; hence $Y'_l - Y_l$ is zero for all lines through O, with possibly one exception, and it follows that Y' coincides with Y. Thus we have the general theorem:

Theorem 1. *If $P^{(1)}, Q^{(1)}$; $P^{(2)}, Q^{(2)}$; ... are any pairs of points, if $a_1, a_2, \ldots$ are constants, positive or negative, associated with them, and if l is a variable line through a fixed point O, then with any point X that is distinct from O and any constant k that is not zero is associated one and only one point Y such that $\Sigma a_r P_l^{(r)} Q_l^{(r)} - kX_l Y_l$ is independent of the direction of l.*

That is to say, the sum $\Sigma a_r P_l^{(r)} Q_l^{(r)}$ is expressible in the form $kX_l Y_l + K$, where the points X and Y, the coefficient k, and the remainder K are independent of l. The expression in this form is not as a rule unique, for the point X and the coefficient k are arbitrary. But while X must not be taken at O, it is of course possible that Y may prove to be at O in a particular case. This occurs if the sum $\Sigma a P_l Q_l$ is itself independent of the direction of l; then Y is at O for all positions of X, and K has always the same value. In general the roles of Y and X may be interchanged, and we may think of the points less as derived one from the other than as forming a symmetrical pair connected with the sum $\Sigma a P_l Q_l$ and the constant k, and determining the constant K.

Different pairs X, Y and S, T connected with the same sum and the same constant k are related in accordance with the original lemma: the triangles OXS, OTY are directly similar. To discover whether the sum is expressible in the form $kS_l^2 + K$, a square term S_l^2 taking the place of the product $X_l Y_l$, we have only to ask whether T can coincide with S, that is, whether there is necessarily a position of S where the triangles OXS, OSY are themselves directly similar. The simple angles XOS, SOY are equal if and only if S is on the line which bisects internally the simple angle XOY. If S is on this line, the triangles are similar if and only if the length of OS is numerically the geometric mean between the lengths of OX and OY. For any given value

of k there are therefore two self-corresponding points S', S'', one the image of the other in the pivotal point O.

If Y is the point associated with the point X and the constant k, the point associated with the same point X and a different constant h is the point Z in OY such that $hOZ = kOY$, and the self-corresponding points R', R'' associated with the constant h can be determined from X and Z. If the ratio of h to k is positive, the half-line OZ is identical with the half-line OY, and therefore R' and R'' are in the line $S'S''$. But if the ratio of h to k is negative, Z is on the opposite side of O to Y, and the internal bisector of the simple angle XOZ is perpendicular to the internal bisector of the simple angle XOY. Since the numerical value of $OX.OZ$ does not depend on the sign of the ratio of h to k, the distance of R' and R'' from O depends only on the absolute value of h.

THEOREM 2. *For any value of k other than zero, the sum ΣaP_lQ_l, where l is a variable line through a fixed point O, is expressible in the form $kS_l^2 + K$, where S and K are independent of l. Unless the sum is actually itself independent of l, the positions of S for different values of k occupy two perpendicular lines through O, one line corresponding to positive values of k and the other to negative values. To a given value of k correspond two points on the appropriate axis at the same distance from O; points which are at the same distance from O but not on the same axis correspond to values of k which are numerically equal but differ in sign.*

§ 2. To reduce to its simplest form a sum ΣaP_lQ_l when the line l is no longer restricted to pass through a fixed point, we first, as in a number of other investigations, associate with l the parallel line through a point O, as yet unspecified. Denoting this parallel line by m, we have identically $P_l = P_m + O_l$, $Q_l = Q_m + O_l$, and therefore

$$\Sigma aP_lQ_l = \Sigma aP_mQ_m + \Sigma a(P_m + Q_m).O_l + \Sigma a \,.\, O_l^2.$$

If Σa is not zero, then $2\Sigma a$ is not zero, and the set of loaded points formed by associating a_r with both $P^{(r)}$ and $Q^{(r)}$ has an accessible mean centre; if O is placed at this point, then $\Sigma a(P_m + Q_m)$ is zero for every line m through O. Thus with this choice of O,

$$\Sigma aP_lQ_l = \Sigma aP_mQ_m + \Sigma a \,.\, O_l^2.$$

But since Σa is not zero and m passes through the fixed point O, there are two points S', S'' which allow ΣaP_mQ_m to be expressed in the form $-\Sigma a \,.\, S_m^2 + A$, where A is independent of m, and we have

$$\Sigma aP_lQ_l = \Sigma a \,.\, (O_l^2 - S_m'^2) + A = \Sigma a \,.\, (O_l + S_m')(O_l + S_m'') + A = \Sigma a \,.\, S_l'S_l'' + A.$$

THEOREM 3. *If $P^{(1)}$, $Q^{(1)}$; $P^{(2)}$, $Q^{(2)}$; ... are any pairs of points, and if a_1, a_2, are constants, positive or negative, associated with them, then provided only that the sum of the constants is not zero, there is a pair of points S', S'' such that for every line l in the plane*

$$\Sigma a_r P_l^{(r)} Q_l^{(r)} = \Sigma a_r \,.\, S_l'S_l'' + A,$$

where A is independent of l.

The construction we have employed leads definitely to a particular pair of points S', S'', and we have to ask whether another construction can lead to another pair of points in terms of which the sum ΣaP_lQ_l takes an equally simple form, in other words, whether we can have $aS_l'S_l'' + A = bT_l'T_l'' + B$ identically for all positions of l. Taking l to contain both S' and T', we have $A = B$, and the identity reduces to $aS_l'S_l'' = bT_l'T_l''$; from this identity it follows that every position of l which contains either S' or S'' contains either T' or T'', and *vice versa*, and therefore that T' and T'' are the same two points as S' and S'', and lastly by taking for l any line which does not contain S' or S'', we have $a = b$. That is to say, if l is entirely unrestricted in the plane, there can not be more than one expression of the sum ΣaP_lQ_l in the form

kX_lY_l+K, and the only expression of this form that can exist when Σa is not zero is the one to which our construction has led.

It remains to consider the reduction in the special case in which Σa is zero, which is the parabolic case. For any position of O,

$$\Sigma aP_lQ_l=\Sigma aP_mQ_m+\Sigma a\,(P_m+Q_m)\,.\,O_l,$$

but just because Σa is zero, we can do nothing to simplify the second term. If we move the origin from O to O', substituting $P_{m'}+O'_m$, $Q_{m'}+O'_m$ for P_m, Q_m, we have

$$\Sigma a\,(P_m+Q_m)=\Sigma a\,(P_{m'}+Q_{m'})+2\Sigma a\,.\,O'_m=\Sigma a\,(P_{m'}+Q_{m'}),$$

and the coefficient of O'_l has the same value as the coefficient of O_l. Otherwise expressed, because $2\Sigma a$ is zero, the vector $\Sigma a\,(OP+OQ)$ is independent of the position of O. If this vector is null, the properties of a centroid belong, accidentally as we may say, to every point of the plane; the set of loaded points can be described as a null set. But this is the exceptional case. Because the sum of the loads in the set is zero, the set has a definite vector β, and if OB is the step from O which has this vector, then $\Sigma a(P_m+Q_m)=B_m$ for every line through O. Thus we have

$$\Sigma aP_lQ_l=\Sigma aP_mQ_m+B_mO_l.$$

The point B depends on O, but the vector OB is independent of O, so that if B and O are distinct for one position of O, they are distinct for every other position of O.

If B is distinct from O, there is a point S such that ΣaP_mQ_m has identically the form B_mS_m+A, where A is independent of the direction of m. Substituting this expression for ΣaP_mQ_m, we have

$$\Sigma aP_lQ_l=B_m(S_m+O_l)+A=B_mS_l+A.$$

The coefficient B_m is not really dependent on O, for it is the projection of the constant vector β in the direction in which S_l is measured. If f is any constant other than zero, and if F is the point such that the vector fFS is β, then $B_m=fS_n$, where n is the line parallel to l through the fixed point F.

THEOREM 4. *If $P^{(1)}$, $Q^{(1)}$; $P^{(2)}$, $Q^{(2)}$; ... are any pairs of points, and if a_1, a_2, ... are constants associated with them, then if the sum of the constants is zero but the set of loaded points formed by attaching each number to both the points in the corresponding pair is not a null set, and if f is any constant other than zero, there are points S, F such that for every line l in the plane*

$$\Sigma a_rP_l^{(r)}Q_l^{(r)}=fS_nS_l+A,$$

where n is the line through F parallel to l, and A is independent of l.

Arguing as before we easily prove that the value of A and the position of S are unique, and that the vector fSF is unique also. The only variation possible in the expression is in the position of the point F, which may be brought to any point on a determinate line through S, except S itself, by an appropriate choice of f.

If Σa is zero and if $\Sigma a(P_m+Q_m)$ is zero also, for all directions of m, we have

$$\Sigma aP_lQ_l=\Sigma aP_mQ_m.$$

That is, the sum ΣaP_lQ_l has the same value for l as for the parallel line m, or, in other words, depends only on the direction of l. There are no accessible points related in any special manner to the sum, but we can examine the sum by taking any origin and attending only to lines through that origin. Reading the terms X_l, Y_l of Th. 1 as projections of the vectors of OX and OY, we may enunciate as follows:

THEOREM 5. *If the loaded set formed from a number of pairs of points $P^{(1)}$, $Q^{(1)}$; $P^{(2)}$, $Q^{(2)}$; ... by attaching a constant a_1 to both $P^{(1)}$ and $Q^{(1)}$, a constant a_2 to both*

$P^{(2)}$ and $Q^{(2)}$, and so on, is a null set, then with any proper vector ξ and any constant k that is not zero is associated one and only one vector η such that for every line l in the plane

$$\Sigma a_r P_l^{(r)} Q_l^{(r)} = k\xi_l\eta_l + K,$$

where ξ_l, η_l denote the projections of ξ, η perpendicular to l, and K is independent of l.

That the projection of a fixed vector may replace the distance from a fixed point, not only in the conclusion of Th. 3, converting this result into Th. 4 or Th. 5, but also in the sum ΣaP_lQ_l which is to be reduced, is obvious. We can avoid direct examination altogether by remarking that any projection μ_l can be replaced by $M_l'' - M_l'$, where $M'M''$ is any step whose vector is μ. A mixed sum $\Sigma aP_lQ_l + \Sigma bR_l\lambda_l + \Sigma c\mu_l\nu_l$ can therefore be replaced at once by a sum of the homogeneous form ΣaP_lQ_l. But except for the sake of brevity we might have included projections of fixed vectors throughout. In § 1, where the variable line l passes through a fixed point O, there is no difference between the perpendicular on l from a fixed point L and the projection normal to l of the fixed vector OL. In the transformation with which § 2 begins, we shall have, replacing λ_l, μ_l, ν_l by λ_m, μ_m, ν_m to emphasise that these projections are determinable from m without reference to l,

$$\Sigma bR_l\lambda_l + \Sigma c\mu_l\nu_l = \Sigma b\lambda_m \,.\, O_l + \Sigma bR_m\lambda_m + \Sigma c\mu_m\nu_m.$$

The coefficient of O_l^2 is unchanged, and therefore the presence of vectorial terms does not affect the criterion distinguishing the parabolic case, but the condition for the sum to depend only on the direction of l is now

$$\Sigma a(P_m + Q_m) + \Sigma b\lambda_m = 0:$$

the set formed by loading the points P and Q has no longer to be null, but to have a vector $-\Sigma b\lambda$. The sum $\Sigma aP_mQ_m + \Sigma bR_m\lambda_m + \Sigma c\mu_m\nu_m$ is homogeneous in fact, though not in appearance, and the application of Th. 2 follows as before.

With regard to the bearing of the theorems that have been enunciated on the identification of the envelope of a line for which a sum ΣaP_lQ_l, or more generally a sum $\Sigma aP_lQ_l + \Sigma bR_l\lambda_l + \Sigma c\mu_l\nu_l$, has a given constant value, the only remark that is perhaps not wholly superfluous is that the degenerate cases of the envelope are presented in an unusually rational light. As special cases of a family satisfying such a condition, we have not only a family to which l belongs if and only if l passes through one or other of the two fixed points S', S'', but a family to which l belongs if l either passes through a fixed point S or is parallel to a fixed vector β, and, lastly, a family to which l belongs if l is parallel to one or other of two fixed vectors ξ, η.

§ 3. A simple example of the process of § 2 will illustrate the alternative between two perpendicular axes in the application of the reduction of § 1. With two fixed points A, B, let us consider the simplification of

$$aA_l^2 + 2hA_lB_l + bB_l^2,$$

where a, h, b are given constants; here

$$P^{(1)} = Q^{(1)} = A,\ a_1 = a;\ P^{(2)} = A,\ Q^{(2)} = B,\ a_2 = 2h;\ P^{(3)} = Q^{(3)} = B,\ a_3 = b.$$

If $a + 2h + b \neq 0$, the mean centre of loads $2a + 2h$ at A and $2h + 2b$ at B is the point C in AB which is such that $(a+h)\,CA + (h+b)\,CB = 0$, and therefore such that for any line m through C, $(a+h)A_m + (h+b)B_m = 0$. For any line l,

$$\begin{aligned} aA_l^2 + 2hA_lB_l + bB_l^2 &= a(A_m + C_l)^2 + 2h(A_m + C_l)(B_m + C\ ) + b(B_m + C_l)^2 \\ &= aA_m^2 + 2hA_mB_m + bB_m^2 + (a + 2h + b)C_l^2. \end{aligned}$$

But we have

$$\frac{AB^2}{(a+2h+b)^2}=\frac{CA^2}{(h+b)^2}=-\frac{CA.CB}{(h+b)(a+h)}=\frac{CB^2}{(a+h)^2}$$
$$=-\frac{aCA^2+2hCA.CB+bCB^2}{(a+2h+b)(h^2-ab)}.$$

According as h^2-ab is positive or negative, the quotient

$$(aCA^2+2hCA.CB+bCB^2)/(a+2h+b)$$

is negative or positive.

If the quotient is negative, there are two points S', S'' in AB such that for each of them the quotient has the value $-CS^2$; that is, in this case,

$$aCA^2+2hCA.CB+bCB^2=-(a+2h+b)CS^2,$$

and since S is in AB,

$$aA_m^2+2hA_mB_m+bB_m^2=-(a+2h+b)S_m^2,$$

whence identically

$$aA_l^2+2hA_lB_l+bB_l^2=(a+2h+b)(C_l+S'_m)(C_l+S''_m)$$
$$=(a+2h+b)S'_lS''_l.$$

If Y is the foot of the perpendicular from one of the points S', S'' on l, and if m cuts SY in U, we have

$$C_l^2-S_m^2=UY^2-SU^2=UY^2+CU^2-CS^2=CY^2-CS^2,$$

so that the reduction may be given also in the form

$$(a+2h+b)(aA_l^2+2hA_lB_l+bB_l^2)=(a+2h+b)^2CY^2-(h^2-ab)AB^2.$$

If $(aCA^2+2hCA.CB+bCB^2)/(a+2h+b)$ is positive, and if T is a point in AB such that CT^2 is equal to this quotient, then

$$aA_m^2+2hA_mB_m+bB_m^2=(a+2h+b)T_m^2.$$

If S is either of the points in the line through C perpendicular to AB whose distance from C is the same as that of T, and if m cuts the perpendicular SY from S on l in U, we have in this case $T_m^2=CU^2$, and therefore

$$aA_l^2+2hA_lB_l+bB_l^2=(a+2h+b)CY^2.$$

Alternatively, since $S_m^2+T_m^2=CS^2$,

$$aA_l^2+2hA_lB_l+bB_l^2=(a+2h+b)(C_l^2-S_m^2+CS^2),$$

whence

$$(a+2h+b)(aA_l^2+2hA_lB_l+bB_l^2)=(a+2h+b)^2S'_lS''_l+(ab-h^2)AB^2.$$

§ 4. The converse of the problem with which we have been dealing is the determination of relations of the form $\Sigma aP_lQ_l=$constant satisfied by the tangents to a given conic. Simple relations are to be found by mere decomposition: if S' is the mean centre of $P^{(1)}$, $P^{(2)}$, ..., $P^{(m)}$ for loads α_1, α_2, ... α_m, and S'' is the mean centre of $Q^{(1)}$, $Q^{(2)}$, ... $Q^{(n)}$ for loads β_1, β_2, ... β_n, then the relation $S'_lS''_l=a^2(1-e^2)$ is equivalent to

$$\Sigma\Sigma\alpha_r\beta_sP_l^{(r)}Q_l^{(s)}=\Sigma\alpha_r\Sigma\beta_sa^2(1-e^2).$$

Results of this kind are not necessarily trivial or obvious.

For example, if S is one focus and X is the foot of the corresponding directrix, the other focus is the mean centre of S and X for loads $1+e^2$ and $-2e^2$; hence for any tangent,

$$(1+e^2)S_l^2-2e^2S_lX_l=e^2SX^2,$$

a formula that remains true for the parabola. More importantly, since S' and S'' can be expressed as mean centres in any triangle, any conic can be represented by a relation between the distances l, m, n of its tangents from

the vertices of an arbitrary triangle of reference. In virtue of an identical relation between l, m, n, the equation of the conic can be put into the homogeneous form

$$Al^2+Bm^2+Cn^2+2Fmn+2Gnl+2Hlm=0,$$

and we can recognise in the equations giving the centre of the conic an identification of the centre as the mean centre of the triangle for loads $A+H+G$, $H+B+F$, $G+F+C$; thus a familiar analytical result is seen to be a special case of a property of the centre used incidentally in § 2 above.

Decomposition of loaded foci must give a sum in which each P is combined potentially with each Q in turn, though it may happen that a particular coefficient is zero. Sums of a more general kind are easily found by means of undetermined coefficients, but we can hardly expect the geometrical meaning of the linear relation between seven products of the form P_iQ_i to be obvious at a glance.

T. McHUGH.
E. H. NEVILLE.

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569. [The word "astrolabe" as used] by any of the writers in the middle ages merely meant a stereographic projection.—De Morgan, *Stereographic*, P.C. 23, p. 41.

570. [Richard Watson, Bishop of Llandaff, second wrangler, 1759] (he says himself he ought to have been first) . . . was rather vain of being spoken of as Professor αὐτοδιδακτός, the self-taught professor, or rather the professor who was indebted for what he knew neither to masters nor books.—*R. Watson*, P.C. 27, p. 134.

571. It is said that, when only six years of age, [James Watt] was discovered solving a geometrical problem on the hearth with a piece of chalk.—*Watt*, P.C. 27, p. 136.

572. Algebra generally furnishes some proof of the absurdity of the conditions of a problem when they contradict one another: but this is not the case with geometry. A latent assumption which restricts the generality of a solution always produces its effect in the former science; whereas in the latter such an assumption might be made part of a demonstration, and produce its consequences, without pointing out that those consequences are not true of the general figure which was drawn. The accurate use of the ruler and compasses will sometimes correct an error of this sort . . . *but not always*: solutions have been proposed before now which give so nearly the third part of an angle, that ordinary drawing will not serve to detect their falsehood. Anyone who imagines he has discovered a geometrical trisection should take care to submit his construction to an algebraical verification; that is, if any person possessing algebra enough to do so should ever be in such a case.—De Morgan, *Trisection of the Angle*, P.C. 25, p. 260.

573. Some enterprising writers on arithmetic profess to multiply magnitude by magnitude; and to make their doings more striking, they generally choose for their instance to multiply £99 19s. 11¾d. by £99 19s. 11¾d. . . . The confusion which is caused by the improper use of the concrete unit can hardly be conceived by any but one who has been used to teaching.—De Morgan, *Unit or Unity*, P.C. 26, p. 5.

574. The term "tangents" was not introduced until after the time of [Regiomontanus]. Both by him and Purbach, as by the Arabs, they were called "shadows," the length of the shadow of every object cast by the sun being in fact the tangent of the sun's zenith distance, the radius being the vertical height of the object.—De Morgan, *Regiomontanus*, P.C. 19, p. 360.

THE FINGER METHOD OF MULTIPLICATION AND EXTENSIONS OF IT.

By A. Lodge, M.A.

We are greatly indebted to the correspondents who have supplied so much valuable information regarding the history and development of the finger-method of multiplication, and to our Editor for the summary of them which will be found in the notes in this number of the *Gazette*.

Nowadays we hardly need assistance in multiplying factors between 6 and 10, but it will be quite interesting to see that the fingers can be used for factors between 11 and 15, and also for those between 16 and 20. In the former case, only the upper fingers are used, down to the touching fingers, the fingers (starting with the thumbs) being numbered from 11 to 15. The tens are given by the sum, $(e+e')$, and the units by the product, (ee'), of those fingers, and 100 is added to complete the product; the formula being

$$(10+e)(10+e')=100+10(e+e')+ee'.$$

For the later teens, the fingers are labelled from 16 to 20, and the tens are given by twice the sum of the upper fingers, the units being the product of the remaining fingers on each hand, and 200 is added to complete: the formula being

$$(15+e)(15+e')=200+20(e+e')+(5-e)(5-e').$$

But my special object now is to direct careful attention to Robert Recorde's method with which the summary concludes. This method can be extended to include all the other methods, and in fact to supersede them all, though its extension can hardly be called a "finger" method as it transcends all possible use of the fingers.

Recorde's example takes the two factors 8 and 7, writes down the complementary numbers 2 and 3 which are required to make up the scale number 10, and then takes the cross-difference between either of the given factors and the complement of the other factor: in this case the cross-difference is 5. The product 8×7 is equal to the product of the scale number and cross-difference + the product of the complements, viz.: $10\times5+2\times3=56$.

Recorde arranges the work in the form of a cross to emphasize the need of *cross* differences, but so long as we remember this need it might be more convenient to write

$$\left.\begin{matrix} 8\times7 \\ =\ 2\times3 \\ +10\times5 \end{matrix}\right\}=56$$

The extension which I advocate is based on the fact that there is no need to take 10 as the scale number: any convenient number will do equally well. Thus, let s be the scale number, and a, b the given factors. The work is then

$$\left.\begin{matrix} a\times b \\ =(s-a)(s-b) \\ +s(a+b-s) \end{matrix}\right\}$$

As a rule, in practice, the complements $s-a$, $s-b$ should be small, so that the method is most useful when a, b are not very different. Perhaps the process is best illustrated by examples:

(1) $$\left.\begin{matrix} 17\times17 \\ =\ 3\times\ 3 \\ +20\times14 \end{matrix}\right\}=289$$

(2) $$\left.\begin{matrix} 13\times13 \\ =\ 3\times\ 3 \\ +16\times10 \end{matrix}\right\}=169$$

(3) $$\left.\begin{matrix} 37\times34 \\ =\ 3\times\ 6 \\ +40\times31 \end{matrix}\right\}=1258$$

(4) $$\left.\begin{matrix} 43\times34 \\ =-3\times\ 6 \\ +\ 40\times37 \end{matrix}\right\}=1462$$

In these examples we have made either the scale number or the cross-difference to be a multiple of 10, this being of the essence of the simplification; but in simplifying fractions, or working in shillings and pence, etc., some other scale number may be more convenient, as in the following instances:

(5) $\left.\begin{matrix} 37\times 33\text{d.} \\ =-1\times\ 3 \\ +\ 36\times 34 \end{matrix}\right\} = 101\text{s. } 9\text{d.} = £5\ 1\text{s. } 9\text{d.}$

(6) $\left.\begin{matrix} 13\times 14\text{ oz.} \\ =\ 3\times\ 2 \\ +16\times 11 \end{matrix}\right\} = 11\text{ lb. } 6\text{ oz.}$

(7) Simplify $\frac{37\times 34}{19}$.

Here, having an eye to the denominator, we will take 38 as the scale number for the numerator. Thus:

$$\left.\begin{matrix} 37\times 34 \\ =\ 1\times\ 4 \\ +38\times 33 \end{matrix}\right\} \quad \therefore \text{ the fraction} = 66\tfrac{4}{19}.$$

There is, please notice, no need to multiply out the numerator.

(8) Simplify $\frac{137\times 134}{129}$.

Work
$\left.\begin{matrix} 137\times 134 \\ =-8\times -5 \\ +129\times 142 \end{matrix}\right\}$

Result $= 142\frac{40}{129}$.

This is almost magical!

Its efficacy rather depends on the numbers with which we have to deal being somewhat near each other, or at any rate simple multiples of them having that nearness. So we must consider the method as not of universal application: just a reserve method to be used on appropriate occasions. Most probably anyone who cares about such methods may devise extensions of it to suit himself, and I have used such extensions in cases where the factors are far from being equal. It is enough here to point out how useful the method may be on occasion.

Finally, I would like to point out that the formula used is merely an extension of the formula for the difference of two squares; $a^2-b^2=(a+b)(a-b)$.

If the factors are equal, it is this formula which we do actually use. Thus:

$$\left.\begin{matrix} 17\times 17 \\ =\ 3\times\ 3 \\ +20\times 14 \end{matrix}\right\} = 289 \text{ is equivalent to } 17^2-3^2=(17+3)(17-3).$$

The extended formula is: the product of two numbers – the product of the two complementary numbers = the product of (the sum of one of these numbers and its *own* complement) by (the difference between this same number and the *other* complement); *i.e.* = the *direct* sum × the *cross* difference.

ALFRED LODGE.

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575. James Dodson's "Calculator" (1747) is correct to every figure as far as we have thought it necessary to examine it. The author [grandfather of De Morgan] generally corrected errata with his own pen in every copy, and the one before us has his corrections.—De Morgan, *Regular Figures*, P.C. 19, p. 363.

576. *Stupid* is the word employed by teachers when *children* do not learn, or, which is the same thing, when teachers do not know how to teach . . . no one ever saw a stupid infant.—De Morgan, *Q.J.E.* ii. p. 52.

577. We are advocates for the use of many words which have gradually glided out of our books; such, for example, as *minuend, subtrahend, resolvend*, etc. We would even propose to coin *addend*.—De Morgan, *Q.J.E.* ii. p. 71.

578. Mahan says somewhere that Washington's education was almost confined to geometry, trigonometry and surveying.

MATHEMATICAL NOTES.

904. [X. 1.] *Regula Stultorum.*

The letter of Prof. Alfred Lodge in the *Gazette*, vol. xiii., No. 188, p. 367, has aroused considerable interest in many of our readers. A short summary of the correspondence on the subject may be useful to students who desire to carry the investigation further.

Prof. Norman Anning (Ann Arbor University) draws attention to an article in the *American Mathematical Monthly*, Jan. 1915, by Mr. L. J. Richardson, of the University of California, on "Digital Reckoning among the Ancients." Here accounts are given of the early use of the fingers to represent numbers, from 1 to 10,000. The methods date back to the Egyptians and Babylonians. Reference under δακτύλος in Liddell and Scott, and under *digitus*, 1.B., in Lewis and Short, will illustrate the use of the fingers to represent numbers, to count, and to reckon. In the section on the Four Operations, the author states that new evidence on the history of multiplication has come to light. Under Trajan, Dacia (roughly the modern Roumania and Transylvania) becomes a Roman province. "The Wallachian peasants, who dwell in Southern Roumania, still preserve an old method of multiplication on the fingers."

The method mentioned by Canon Wilson may be symbolised as

$$(5+e)(5+e')=10(e+e')+(5-e)(5-e'),$$

where e, e' are the number of fingers used on the right and left hands respectively, counting the thumb as the first of them, and $5-e$, $5-e'$ are the remaining fingers.

The author gives extensions of this formula, with a simpler formula applicable to factors in the early half of each decad of numbers, *e.g.*

$$(10+e)(10+e')=100+10(e+e')+ee',$$
$$(15+e)(15+e')=200+20(e+e')+(5-e)(5-e'),$$
$$(20+e)(20+e')=400+20(e+e')+ee',$$
$$(25+e)(25+e')=600+30(e+e')+(5-e)(5-e'),$$

and so on, two distinct sets, all of which can be exemplified on the fingers.

He also gives ample references and a short bibliography. The number can be obtained from *The Mathematical Association of America*, Menasha, U.S.A.

The Rev. H. Poole, of Sedbergh School, suggests that Prof. Lodge's rule would have been available for a giant of Gath,* with a six-fingered hand, and presumably with a duodecimal scale.

Prof. Lodge remarks that the identity

$$(a+x)(a+y)=2a(x+y)+(a-x)(a-y)$$

requires $a=5$ for a five-fingered hand and $a=6$ for a six-fingered hand, and would require $a=10$ for a 10-fingered hand.

Thus, in scale of twenty, and a 10-fingered hand,

ꝏ × ꝏ would give us for first digit $5+8=$ ꝏ,

and for the second, $(10-5)(10-8)=5\times 2=t$, *i.e.* ꝏ t.

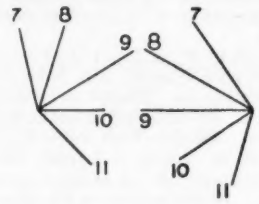
The scale of twenty (especially for two digit numbers only) is so easily converted to the decimal scale that this may be used as the basis of yet another simple method of multiplying "teens"; thus:

$$15\times 18=20(5+8)+(10-5)(10-8)=260+10=270.$$

* II. Sam. xxi. v. 20.

It is easy to reckon in dozens, *i.e.* (say) shillings and pence, anything between 7×7 and 11×11 inclusive, by merely reckoning the two touching fingers in the thumb sets and also in the residue sets.

Thus, if the thumb is 7 and the little finger 11, such a number as 9×8 is obtained by touching No. 9 on one hand with No. 8 on the other, as in



figure; we have then 5 fingers in all on the upper side, giving 5 dozen, and 3 times 4 below, giving 5 dozen, and 12, *i.e.* 6 doz.

Similarly, by merely touching the corresponding fingers we have

$$9 \times \ 9\text{d} = 6 \text{ dozen} + 9 = 6/9,$$
$$9 \times 10\text{d} = 7 \text{ dozen} + 6 = 7/6,$$
$$10 \times 11\text{d} = 8 \text{ dozen} + 4 = 8/4, \text{ etc.},$$

and easiest of all, $\quad 7 \times \ 7\text{d} = 2 \text{ dozen} + 5 \times 5 = 4 \text{ dozen} + 1 = 4/1,$

and here the two thumbs touch.

Miss F. A. Yeldham compares such methods with the old practice as illustrated by Sacrobosco and Recorde.

From the time of the *Carmen de Algorismo* of Alexander de Villa Dei, in 1220, to the sixteenth century the rule is based on *differences*. In the Algorism of Sacrobosco it runs:

"quinam digitus multiplicat digitum, subtrahendus est minor digitus ab articulo suae denominationis per differentiam majoris digiti ad denarium, denario simul computato."

An example is given: 4 multiplied by 8 is equal to $4\,(10 - 2)$,

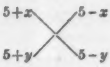
i.e. $40 - 8$, or 32.

The following is from the 1582 edition of Robert Recorde's *Grounde of Artes*:

"As for the small digits under 5, it were but folly to teach any rule seeing they are so easy, that every child can do it: but for the multiplication of the greater digits, thus shall you do. First set your digits one over the other right, then from the uppermost downwards, and from the nethermost upwards, draw straight lines, so that they make a cross, commonly called Saint Andrew's cross, as you see here. Then look how many each of them lacketh of 10, and write that against each of them at the end of the line, and that is called the Differences. . . .

". . . Last of all I multiply the two differences, saying 2 times 3 make 6, that must I ever set under the differences, beneath the line; then must I take the one of the differences (which I will, for all is like) from the other digit (not from his own) as the lines of the cross warn me, and that that is left, must I write under the digits. As in this example, if I take 2 from 7, or 3 from 8, there remaineth 5; that 5 must I write under the digits and then there appeareth the multiplication of 7 times 8 to be 56."

"Which I will, for all is like" is a discovery for one who would not readily see the general case,



which leads to the formula

$$(5+x)(5+y)=(x+y)10+(5-x)(5-y)$$

as the finger method does.

905. [K.[1] 14.] In the *Encyclopedia Britannica* (11th edit.), vol. 22, p. 28, article "Polyhedron," the rhombi-cube octahedra are incorrectly described. The small one is given as a polyhedron bounded by 12 pentagons, 8 triangles and 6 squares. The actual figure is bounded by 18 squares and 8 triangles. The large one is described as a polyhedron bounded by 12 decagons, 6 squares and 8 triangles, while the actual figure possesses 12 squares, 6 octagons and 8 hexagons.—M. Goldberg [*American Math. Monthly*, March 1928, p. 136].

PROBLEMS AND THEIR SOLUTIONS.

THE number of applications to individual Officers of the Association for solutions of mathematical problems is now so considerable as to give reason for the belief that many members would appreciate the formation of something akin to a Problem Section to which all such enquiries may be sent.

The matter is now under the consideration of the Council, but in the meantime we are glad to say that applications for such assistance in the solution of problems may be addressed to

A. S. GOSSETT-TANNER, Esq., M.A., Derby School, Derby,

who has most kindly consented to act as the temporary Director of such a Section.

He will be glad to receive the names of members, especially of those engaged in scholarship or similar work, who are willing to volunteer assistance when called upon. Matters will be facilitated if such members will kindly indicate the subjects upon which they prefer to be consulted.

Those who require the help of the Section should, wherever possible, state the source of their problems, the nature of their difficulty, and the names and authors of the text-books (or "keys") on the subject which they possess.

It is hoped that it will be unnecessary to make any charge to applicants who are members of the Association, but at present they are requested to send a *stamped* and *addressed* envelope for the reply.

It may be found possible to print in the *Gazette* solutions of unusual interest or elegance.

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579. So much for the system of Arithmetic, most appropriately called *cyphering*, since intellect goes for nothing throughout.—De Morgan, *On Mathematical Instructions*, *Q.J.E.* ii. p. 268.

580. It is curious enough that some older mathematicians (among the rest, the illustrious Cocker) call fractions *negative* numbers, and seem not to be very clear about admitting them at all.—De Morgan, *Q.J.E.* ii. p. 109.

581. With children a preparatory course of experimental geometry is the best introduction to the severer study.—De Morgan, *Q.J.E.* ii. 95.

582. On the rule of false position [Recorde] was in the habit of astonishing his friends by proposing difficult questions, and working the true result by taking the chance answers of "suche children or ydeotes as happened to be in the place."—De Morgan, *Recorde*, P.C. 19, p. 331.

REVIEWS.

Die Grundlagen der ägyptischen Bruchrechnung. By O. NEUGEBAUER. Pp. 45. RM. 7.50. 1926. (Springer, Berlin.)

This volume contains an interesting account of the methods of manipulating rational fractions known to the ancient Egyptians. Several books on this subject have been published in the last twenty years, and the field is now fairly well explored. Mathematical advancement in Egyptian times was hampered by the notation existing then. In arithmetic, the distinction between prime and composite numbers was appreciated by the Greeks only at a later period. The solution of an arithmetical problem by Egyptian methods was rather like that of a present-day puzzle in which the methods permitted to be used are severely restricted by conditions. Until the ordinary decimal notation for numbers came into being about the same time as the invention of printing, progress in arithmetic was necessarily slow. Dr. Neugebauer's book contains a useful bibliography of literature on the subject, with several tables requisite for dealing with fractions by Egyptian methods.

W. E. H. B.

(I.) **Dynamical Systems.** By D. BIRKHOFF (American Mathematical Society Colloquium Publications, Volume IX.). Pp. viii + 295. 1927.

(II.) **A Treatise on the Analytical Dynamics of Particles and Rigid Bodies.** By E. T. WHITTAKER. *Third Edition.* Pp. xiv + 456. 25s. net. 1927. (Cam. Univ. Press.)

In any branch of Applied Mathematics we can usually recognise two more or less distinct stages: firstly the reduction of a problem to mathematical terms, and secondly the subsequent developments which are a matter of pure mathematics. In the case of Dynamics this distinction is very marked; General Dynamics as it has been developed during the past century is simply the theory of differential equations of Lagrangian or Hamiltonian form. Of the books under review Birkhoff's *Dynamical Systems* deals almost entirely and Whittaker's *Analytical Dynamics* deals largely with this "higher" aspect of Dynamics.

The central problem of General Dynamics to-day is that of the nature of the solution of dynamical equations, *i.e.*, systems of Lagrangian or Hamiltonian form. This problem had its origin in Celestial Mechanics, where in order to prove the stability of the solar system it was desired to solve the equations of motion by means of purely trigonometric series. This was conjectured by Laplace, but the clear proof of its possibility under certain conditions had to wait for Poincaré; Poincaré, however, brought forward evidence tending to show that the series in question are in general divergent, so that the desired inference about the solar system could not be drawn. There is no doubt that the problem that lies behind this divergence is one of extreme complication. Advance has, however, been made in two directions. From the analytic side, though we cannot obtain the *general* solution in the form of convergent trigonometric series, we can prove the convergence of such series specifying certain *particular* solutions; in particular the theory of Periodic Solutions was established on a firm basis by Poincaré and has been extended by Whittaker and by Birkhoff and his pupils. Again we may renounce the idea of obtaining convergent series for our solutions and enquire instead into certain qualitative properties, such as are contemplated in the idea of stability of motion. The first step in this direction was taken by Poincaré when he proved the existence under certain conditions of "quasi-periodic" solutions in which the initial configuration is regained to within an arbitrarily small distance provided the motion is followed for a sufficiently long time; and here again Birkhoff has made notable advances.

(I) Up to the present time the student of these matters has had no convenient and readable book to which to turn for information. Practically the only source has in fact been *Les Méthodes Nouvelles de la Mécanique Céleste,*

but that great work of Poincaré contains much that is obscure, and much that is—for a student of General Dynamics—unessential complication peculiar to the details of Celestial Mechanics. The publication of Birkhoff's *Dynamical Systems* is therefore specially welcome, for it deals in a systematic way with precisely these problems. The book is an amplification of the Colloquium lectures delivered before the American Mathematical Society in September, 1920. It is a well-established tradition of these Colloquia that the lecturer shall give particular emphasis to his own researches, and Birkhoff is the one man who could have produced a balanced account of this subject while keeping to this tradition. Nevertheless one cannot help feeling a regret that he did not expand his book by including a critical account of such earlier work as Poincaré's theory of periodic and asymptotic solutions.

This is most certainly a book to stimulate research in the fascinating field.

(2) Whittaker's *Analytical Dynamics* has long been recognised as the standard advanced text-book in this subject, and its continued popularity is shown by the demand for a third edition, which has now appeared. The bulk of the book (Chapters I.-XIV.) has been reproduced photo-lithographically from the second edition, with some corrections and additional references. This portion comprises a systematic treatment of the usual field of Particle- and Rigid-Dynamics, the older part of General Dynamics (variational principles, theory of contact-transformation, etc.), and an introduction to the Problem of Three Bodies. The last two chapters are concerned with solutions of dynamical equations, and these have been completely re-written in order to take account of recent developments in this field. Chapter XV.—" the general theory of orbits "—deals chiefly with periodic and asymptotic solutions and with questions of stability. For the most part the account given is of an illustrative and introductory nature, and from this point of view it is excellent and is a great improvement on the preceding edition; on the whole, however, the chapter hardly lives up to its title. In Chapter XVI. the author considers the formal solution of Hamiltonian systems by means of trigonometric series, and has replaced his former method, as given in the preceding edition, by a new one which he first published in 1916 (*Proc. Royal Soc. Edin.* " On the adelphic integral of the differential equations of dynamics "). The account must be regarded as suggestive rather than conclusive, for a process is given for constructing the " adelphic integral " series in a certain form without the necessary proof that it will be successful. One must criticise too the optimistic view taken by the author of the convergence of the series; though the question is a difficult one, all the evidence suggests that the series are generally divergent and only exceptionally convergent.

T. M. CHERRY.

The Logarithmic Potential. By G. C. EVANS. Pp. viii, 150. $2. 1927. (American Math. Soc.)

This is a book which brings together a number of researches carried out by the author. The chief aim is a study of the potential and Dirichlet problems in two dimensions—an important and much-handled theme—but Professor Evans has been able to throw new light on it by introducing Stieltjes integrals so as to cover the cases of discontinuous single-layer and double-layer distributions. This method not only widens the field of application so as to approximate more nearly to modern physical theories, but enables even the more classical results to be obtained with remarkable directness and simplicity. The chief lemma used is Theorem 7, which shows that if the function which refers to the double-layer distribution has at a particular point of the boundary a unique derivative along the boundary, the potential function approaches the proper value as the boundary point is reached along any path between two chords.

Certain valuable properties of uniform sequences of functions of limited variation, found in part by Bray, are used skilfully, and good use is made of much modern work on sequences and integrals. Indeed the book may be recommended perhaps more because it illustrates so well the fundamental simplicity of these recent points of view than because of the problem handled, fundamental though that is.

The reviewer warmly welcomes the introduction of a limited number of examples. Even the general reader is helped by them to a clearer understanding, while the advanced student, for whom such a book is specially designed, will find them invaluable. In this particular, and in the careful setting out of theorems to be proved, the book breaks away happily from the style of the Borel tracts, to which in other ways it approaches.

The printing is good and there is a useful index. There is only one, quite minor, criticism which the reviewer desires to make. There is such confusion between the term "summable" as applied to series and to integrals that some mathematicians nowadays prefer to replace "summable" by "integrable" when integrals are being dealt with. Since both concepts are used throughout the book and appear sometimes in the same sentence, the possibility of confusion becomes actual.

The author and the reviewer have exchanged so many criticisms in the past that the last remark may not be wasted.

Differential und Integral rechnung. By R. COURANT. Vol. i. pp. xiv + 410. RM. 18. 1927. (Springer.)

This is intended as an ordinary text-book on the subject in the same class as Lamb or Osgood, except that in this first volume only one variable is considered. In comparison with the above-mentioned books it assumes rather less knowledge from the reader to begin with and devotes more care in illustrating elementary points, but it also goes further into the study of sequences, irrational numbers, Fourier series, and the like. The definition of the integral of a continuous function is given carefully in place of the usual assumption of an integral which is monotonic in stretches. The definition is directly applied to x^2 and to $\sin x$. Similarly, at a very early stage on differentiation the mean-value theorem is introduced.

There is no doubt that greater stress is laid on general theory than would be the case in an English book catering for students of the same type. There is correspondingly less weight given to the manipulation of complicated formulae. For example, in dealing with partial fraction methods of integration, nothing is said about the cases in which factors of the denominator are repeated. Throughout the book there is a constant attempt to consider applications to the experimental sciences and the number and aptness of illustrations and examples is unusual in a continental work. In this particular Courant has happily followed English tradition.

On the whole, though the book does not differ from our standard books very appreciably, there are a number of points here and there which a teacher of prospective or first year honours candidates might well take into consideration in preparing his notes.

The index is good, the figures clear and plentiful. Fortunately the book, like most, but not all, modern German scientific works, is printed in Latin characters. P. J. D.

Vorlesungen über Nicht-Euklidische Geometrie. By F. KLEIN. Revised by W. ROSEMANN. Pp. xii + 326. 18m. Bound 19.50m. 1928. (Springer, Berlin.)

Non-Euclidean Geometry is an instance of a doctrine erected in response to the demands of abstract thought which has eventually had an application in practical science. Arising as a result of the unsuccessful attempts to show Euclid's parallel axiom, it was successively extended, until, a few years ago, it was hoped by some that the whole of physics could be expressed as a geometry sufficiently generalised.

The earliest published works on the subject, by Bolyai and Lobatschefsky, used the "elementary" methods of Euclid, and trigonometry, simply replacing *his* parallel Axiom by *theirs*. The erection of a pure projective geometry by von Staudt enabled Klein to regard the Euclidean geometry, the geometry of Bolyai and Lobatschefsky and the elliptic geometry as derived from projective geometry by the introduction of various metrics therein.

This is the standpoint adopted in the present work, which is a revised edition of the lithographed lectures published in 1892. Account is taken of recent

work which bears on the method used; thus the geometry of the restricted relativity theory falls easily into the framework. As is always the case with Klein's work, imagination is continually demanded, the reader is made to *see* the proofs with the physical as well as with the mental eye, and contact is established with other branches of mathematics, *e.g.* the theory of linear substitutions of the complex variable, automorphic functions and the problem of uniformising multiply-valued functions.

The book first of all deals with projective geometry, including the theory of quadrics, treated algebraically, and of collineations which leave a quadric invariant; then it is shewn how co-ordinates can be introduced without using the idea of distance, and how by adopting various definitions of distance and angle-measure we can reach the classical non-Euclidean geometries. The group concept is of course used throughout. The properties of the Clifford surface prepare the way for the Clifford-Klein problem, which is treated fairly fully in the two dimensional case and sketched for three dimensions. It is emphasized that a locally Euclidean space need not have an infinite volume, but perhaps it is not made quite clear which axioms of Euclidean geometry cease to hold in a space locally Euclidean and of finite volume. It is in just such a case that the axiomatic "elementary" method makes things plain. Probably Klein was not altogether sympathetic to logical investigations in geometry, and in one passage he almost seems to reject the emphasis laid on the axiomatic treatment in recent years. Surely both points of view, axiomatic and intuitive, have their value, are complementary to each other and alike indispensable.

As we have said, Klein treats the subject from the standpoint of projective geometry and deals only with the classical non-Euclidean geometries. The elementary method of the founders is not touched, and only a few pages are devoted to the Differential Geometry method of Riemann. The latter is however found in all books on general relativity and would hardly find a place here. Nor is there any reference to the finite projective geometries of Veblen and his followers, nor to the recent work on Mengenlehre which may lead to general geometries of types quite other than the classical. To have treated these would have destroyed the very evident artistic unity of the book.

The book is one of the Grundlehren series, in which it is intended to republish all Klein's lectures. The unifying tendency of his work is of special value at the present time when mathematics seems again in danger of being broken up into watertight compartments. H. G. F.

The Elements of the Calculus. By W. P. Milne and G. J. B. Westcott. Pp. viii + 92. 3s. 1927. (G. Bell.)

Elementary Differential Calculus. By G. L. Parsons. Pp. xix + 204. 6s. 1927. (Cam. Univ. Press.)

Introduction to the Calculus. By W. F. Osgood. Pp. xi + 449. 12s. net. 1926. (The Macmillan Company.)

The first book on the list is very attractive and entirely bears out the authors' claim of containing in small compass sufficient for the acquisition of a good working knowledge of the Calculus. "No attempt is made to base the fundamental principles upon the modern foundation of arithmetical rigour: only the aid of geometrical intuition is invoked." It is surprising that so much has been comprised in 92 pages, of which 30 are devoted to revision examples and the answers to all the examples. The differentiation of x^n when n is not a positive integer is performed without recourse to the Binomial Theorem, and the geometrical method for differentiating $\log x$ is delightful. Also the method given for differentiating the product or quotient of two functions is an improvement on the usual treatment. The examples are numerous and well graded. The book should be found very useful as a first course.

The writer reviewed Mr. Parsons' *Elementary Integral Calculus* on a former occasion, and his *Elementary Differential Calculus* is intended to be a companion to it. It has the same general purpose, that of the preparation of candidates for the Higher School Examination. The opportunity is taken of again protesting against the plan of separating, in the first course, the two

aspects of one subject, however urgent the claims of an examination may be. Having taken a group of beginners through a first course of the Calculus year after year, the writer testifies that his boys have not found difficulties in a *pari passu* treatment, while examination results have not suffered in the least. There is much more force in the author's second reason for writing the book, that "there is too wide a gap between the easy introductions to the Calculus, which do not go far enough, and the advanced text-books, in which the main principles are in danger of being lost in a mass of subsidiary detail." If only the author had seen fit to combine and rearrange his two books, all needs would have been met.

However, having undertaken to review this book, it is necessary to give some account of it. It opens with an excellent historical sketch, and goes on to cover a great deal of ground. After a chapter on functions, limits and continuity, the usual processes, rules and geometrical applications are given. In the miscellaneous applications to Algebra, Trigonometry and Mechanics there is a weak spot in § 75, where the author logarithmically differentiates the infinite product for $\sin\theta$ without referring at all to the serious difficulties connected therewith! He might have made a remark similar to that in § 68, where he says the reader must be content with a statement of Taylor's and Maclaurin's theorems and the conditions under which they may be assumed to hold. In the preface it is stated that the order of the different parts of the subject is a matter of taste, and most people will prefer to take the chapter on asymptotes before that on simple curve tracing. The latter, for which some originality is claimed, appears to be but simplified "Edwards." The publishers have done their work well. There do not seem to be any misprints; the book is beautifully printed on good paper; there are answers to the examples and an index.

Professor Osgood's *Introduction* is a revision of his First Course. Its objects are to set forth the applications of the Calculus to problems of geometry and mechanics—an easy matter; and to make clear the thought which underlies the Calculus—a most difficult thing to do. As regards the latter object, limits and infinitesimals are treated especially well, with plenty of numerical illustration. In particular it is most refreshing to find the theorem

$$\text{Lim (arc/chord)} = 1$$

definitely proved. It is rather unusual to find so elaborate a chapter on numerical computation in the solution of equations. Very likely the author had his eye on the engineering student, for whom this book was written as well as for the student of pure mathematics. In that case he might have been much fuller in his treatment of the numerical computation of Definite Integrals. Only the trapezoidal rule and Simpson's rule are referred to. It would not have taken many more pages to prove Weddle's rule—a rule of the same kind as the former two—and Dufton's rule, which is of an entirely different kind—not to mention the author's own three ordinate rule (probably entirely useless and for that reason rejected by former discoverers!).

The chapter on Definite Integrals, the most important one in the book, is extraordinarily good. The fundamental theorem of the Integral Calculus is, as usual, suggested by the problem of evaluating an area, and then proved as a piece of analysis. "We may liken the process to that of building a masonry bridge. First a wooden arch is erected. On this are placed the blocks of granite, and when the structure is completed the wooden arch is removed. The bridge is the essential thing, the wood was incidental." This is a picturesque and perfect analogy. Throughout the chapter the method of grinding the limit between an upper and nether millstone is adopted (analogies are infectious), and a great deal of space is given to the discussion and proof of Duhamel's theorem with another delightful illustration from real life. The chapter on Infinite Series contains much that is to be found in any algebra, and it is not better done here, while the sections on computation of logarithms seem to be quite out of place in this book. Naturally after so long a preamble on series one turns eagerly to the treatment of Taylor's theorem, but here the reviewer is helpless because the bookbinder has played him a practical joke. His copy has the pages numbered as follows: 420, 261-276, 437 to the

end; while the table of contents indicates that the most exciting part of the whole story is to be found in the missing pages. The final scrappy chapter on Partial Differentiation might have contained some reference to the extension of Taylor's theorem, especially as half the last page is left blank.

Taken as a whole this is an excellent book, while the publishers have produced it well, with beautifully clear print. Considering the large number of its pages the book is handy, and it lies open flat wherever it is opened. There is an index, and the answer is appended to each example. N. M. GIBBINS.

A Debate on the Theory of Relativity. Pp. viii + 154. $2. 1927. (The Open Court Publishing Company.)

This small book provides very excellent reading for all students of modern physics. Though the actual point at issue is the Theory of Relativity, the arguments of a general nature put forward on both sides are applicable to any physical theory, and could in particular be exemplified just as readily by the recent developments of the Quantum theory.

It is well to draw the attention of the student to the fact that no physical hypothesis is more than an approximation to the truth, and that it is only necessary to push an experiment far enough in order to find out that the laws of physics are imperfectly stated, or that our models of the phenomenon are inadequate: to the difficulties of the interpretations of any experiments which have played a prominent part either in the development or in the verification of a theory: to the unsatisfactory character of all *ad hoc* explanations of experimental results which are so often invented, one for each experiment, without any attempt to harmonize the various explanations: and to the fact that it is never a question of finding the unique explanation of any phenomena, but rather that of deciding which is the most convenient explanation of the infinite number that can be put forward. All these points, together with many others of equal interest and importance, are fully discussed in this book.

Apart from the question as to whether the experimental evidence for the theory of Relativity is conclusive or not, the chief argument of the opponents of the theory is that it upsets all our simple Euclidean notions of space, and our Newtonian idea of time. They claim that it is always preferable to ascribe any failure to verify the theorems of Euclid to physical causes rather than to think in terms of a non-Euclidean geometry with which we can calculate, but for which we have no intuitions, and that "in the long run, our geometries and our mechanics will reflect rather the nature of our intellects than the nature of the Universe which we are trying to understand."

The same difficulty presents itself in any theory. Hence the many attempts to find an interpretation of the quantum which can be used to obtain, on the basis of the electromagnetic theory, results in agreement with experiment rather than remain satisfied with the modern purely mathematical formulation of the Quantum theory. Nevertheless the success of this latter method goes far to justify the feeling of the upholders of the theory of Relativity in their view that people look askance at non-Euclidean geometry merely because they are unfamiliar with it, and not because of any fundamental intuition of the human mind, and that mankind will shortly feel as much at home with the non-commutative algebra of the Quantum theory and the non-Euclidean geometry of the theory of Relativity as with the more familiar algebra and geometry of our present school curriculum. H. R. HASSÉ.

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583. The perversity of human nature, we suppose, lends beginners a really wonderful tact in choosing the wrong idea, if there be two to choose between, that is, in every possible case.—De Morgan, *Q.J.E.* ii, p. 125.

584. It is common enough for a boy to have acquired Arithmetic by rule, six Books of Euclid by rote, the greater part of Bonnycastle's Algebra by rule, and plane trigonometry in the same way, with just enough of the use of a table of logarithms to secure him against working a question with correctness.—De Morgan, *Q.J.E.* ii. p. 266.

THE LIBRARY.

160 CASTLE HILL, READING.

ADDITION.

Professor **M. J. M. Hill**, to commemorate his term of office as President, 1926-1927, has sent copies of his books:

The Fifth and Sixth Books of Euclid	1900
THE SAME {2}	1908
Theory of Proportion	1914

ANNUAL REPORT OF THE QUEENSLAND MATHEMATICAL SOCIETY.

THE Annual Meeting was held at the University on the evening of Friday, March 26th, 1926. The Annual Report and Balance Sheet were presented and adopted. After the election of officers for the ensuing year, Professor Priestley delivered his Presidential Address on "Some Developments in Modern Mathematics."

Three General Meetings have been held. At the first, on Friday, May 14th, Mr. C. R. Campbell, B.A., spoke on "Recreations in Elementary Mathematics" and "The Teaching of Elementary Geometry." At the second, on Friday, August 6th, Mr. R. A. Kerr, M.A., B.Sc., read a paper on "Teaching Elementary Algebra." At the third, on Friday, October 29th, Mr. H. J. Priest, B.A., B.Sc., spoke on the subject "Senior Mechanics."

The number of members of the Society is twenty-seven, of whom thirteen are full members of the Mathematical Association: this membership shows an increase of four. Copies of the *Mathematical Gazette* have been circulated regularly among Associate Members.

The statement of Receipts and Expenditure shows a credit balance of £6 10s., which is the largest to date, and is more than twice the amount in hand at this time last year.

The attendance of members at meetings has been very good, and a keen interest is taken in the activities of the Society.

March 25th, 1927. J. P. MCCARTHY, Hon. Secy. and Treas.

MATHEMATICAL ASSOCIATION.

(LONDON BRANCH).

PROGRAMME FOR 1928–29.

1928.	
Oct. 6th.	"Mathematics and the Difficult Child."—Dr. DODD.
Nov. 10th.	"On Some Graphs."—C. T. DALTRY.
Dec. 8th.	"Aims of Mathematical Education."—J. KATZ.
1929.	
Feb. 2nd.	Presidential Address.—Prof. A. LODGE.
Feb. 23rd.	Annual Meeting—Members' Questions.
Mar. 16th.	"Standard Form."—A. S. GRANT and Miss PUNNETT.

F. C. BOON, *Hon Sec.*

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AT THE UNIVERSITY PRESS, GLASGOW

BOOKS RECEIVED, CONTENTS OF JOURNALS, ETC.

July, 1928.

Birkhoff, G. D. *Dynamical Systems.* (American Mathematical Society. Colloquium Publications, Vol. IX.) Pp. viii+295. 3 $. 1928. (Bowes & Bowes.)

Birtwistle, G. *The Principles of Thermodynamics.* 2nd Edition. Pp. ix+168. 7s. 6d. 1928. (Cambridge University Press.)

Brown, G. W. *Progressive Trigonometry. Part I. Numerical Trigonometry and Mensuration.* Pp. ix+221. 3s. 6d. 1928. (Macmillan.)

Garcia, G. *Teoria general de la flexion de las vigas.* Pp. 21. n.p. 1927. (Imprenta Peruana. Inambari 359 Buenos Ayres.)

Klein, F. *Vorlesungen über Nicht-Euklidische Geometrie.* Edited by **W. Rosemann.** Pp. xii+326. RM. 18. 1928. (Springer, Berlin.)

Pratt, A. S. *Higher Certificate Mathematical Test Papers.* Pp. vi+79. 1s. 6d., with or without answers. 1928. (Methuen.)

Ward, H., and **Roscoe, F.** *The Approach to Teaching.* Pp. x+208. 5s. net. 1928. (Bell.)

Weber, G. *Das Wesen der Materie und der Aufbau der Atome.* Pp. 114. 8 Mk. 1928. (Hillmann, Leipzig.)

Wisdom, A. *Arithmetical Dictation: Systematic Exercises in Mental Arithmetic.* Book V. 1s. 3d. 1928. (Univ. of London Press.)

Abhandlungen aus dem Mathematischen Seminar der Hamburgischen Universität. (Teubner, Leipzig.)

Vol. 6: Nos. 1 and 2, issued Feb. 1928.

Zur Geometrie der Funktionen zweier komplexer Veränderlicher. II. Das Verhalten der Funktionen in der Umgebung ihrer Verzweigungstellen. III. Klassifikation der Singularitäten algebroider Kurven. IV. Die Verzweigungsgruppen. Pp. 1-55. K. BRAUNER. *Ueber Knotengruppen.* Pp. 56-64. K. REIDEMEISTER. *Die Grundlagen der Mathematik.* Pp. 65-85. D. HILBERT. *Discussions on the preceding paper.* Pp. 86-88. H. WEYL. Pp. 89-92. P. BERNAYS. *Bermerkung zur Axiomatik der zweigliedrigen Gruppen.* Pp. 93-95. K. REIDEMEISTER. *Ueber die Geometrie der analytischen Abbildungen die durch analytische Funktionen von zwei Veränderlichen vermittelt werden.* Pp. 96-145. C. CARATHÉODORY. *Die beiden Ergänzungssatze zum Reciprocitätsgesetz der l^n-ten Potenzreste im Körper der l^n-ten Einheitwurzeln.* Pp. 146-162. E. ARTIN and H. HASSE.

American Mathematical Monthly. (Math. Assoc. of America, Menasha.)

Vol. 35: No. 1, Jan. 1928.

Mathematics for Students of Chemistry. Pp. 3-9. F. DANIELS. *A Simplification of certain Problems in Arithmetical Division.* Pp. 9-14. M. WARD. *Formal Unification of Gradient, Divergence, and Curl, by means of an Infinitesimal Operational Volume.* Pp. 14-18. V. KARAPETOFF. *Expressions for the General Determinant in Terms of its Principal Minors.* Pp. 18-21. E. B. STOUFFER. *A Theorem on Algebraic Quadratic Forms and its Application in the General Theory of Relativity.* Pp. 21-24. P. Y. CHOU. *Note on Stereographic Projection.* Pp. 24-27. T. BENNETT. *Some Geometrical Illustrations for the Elementary Course in Differential Equations.* Pp. 27-29. J. D. TAMARKIN. *The origin of the Term "Root" in Chinese Mathematics.* Pp. 29-30. C. C. MA.

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The Mechanical Combination of Linear Forms. Pp. 114-121. D. H. LEHMER. *Strong and weak Inequalities involving the Ratio of two Chords or two Arcs of a Circle: Chains of Inequalities.* Pp. 122-130. H. A. SIMMONS. *The Lowest Common Multiple of n Polynomials.* Pp. 130-132. A. DRESDEN. *Note on the Calculation of Standard Deviations.* Pp. 132-134. V. ROJANSKY. *A Corollary to Cauchy's Integral Formula.* Pp. 134-137. N. MILLER.

American Journal of Mathematics. (Johns Hopkins Press, Baltimore.)

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Additive Number Theory for all Quadratic Functions. Pp. 1-48. L. E. DICKSON. *Démonstration de quelques Propriétés des Ensembles abstraits.* Pp. 49-72. M. FRÉCHET. *On certain points in the Theory of Dirichlet Series.* Pp. 72-86. J. F. RITT. *On a Theorem of Severi.* Pp. 87-92. O. ZARISKI. *On Gierster's Classnumber Relations.* Pp. 93-122. J. V. USPENSKY. *Note on a Theorem of Böcher.* Pp 123-138. G. C. EVANS. *On Taylor's Series admitting the Circle of Convergence as a Singular Curve.* Pp. 139-146. J. J. GREEN and D. V. WIDDER. *A Mathematical Theory of Depreciation and Replacement.* Pp. 147-157. C. F. ROOS.

Bollettino della Unione Matematica Italiana. (Zanichelli, Bologna.)

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Sulle sostituzioni lineari ortogonali. Pp. 1-7. G. VITALI. *Sul calcolo del valore attuale di rendite certe a termini variabili.* Pp. 8-10. F. SIBIRANI. *Osservazioni sulle curve iperellittiche reali.* Pp. 10-14. A. COMESSATTI. *Sulla trasformazione assoluta dei simboli di Christoffel in uno spatio curvo a due dimensioni.* Pp. 14-18. R. TAUCER. *Una configurazione notevole di "reticoli" sopra una superficie dello spazio ordinario.* Pp. 18-24. E. G. TOGLIATTI. *Formula di trasformazione per le valutazioni degli operatori funzionali.* Pp. 25-26. S. L. STRANEO. *La risoluzione apiristica delle congruenze cubiche.* Pp. 27-32. G. SANSONE. *Sulla forma canonica delle matrici.* Pp. 32-34. G. VOGHERA. *Una espressione dei simboli di Riemann di prima specie per le varietà immerse negli spazi euclidei.* Pp. 34-36. A. TONOLO.

Bulletin of the American Mathematical Society. (Bowes & Bowes, Cambridge.)

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Journal of the Mathematical Association of Japan for Secondary Education. (Prof. Iwama, Tôkyô Higher School for Girls.)

Vol. 9 : No. 6, Dec. 1927.

Journal de la Société Physico-Mathématique de Léningrade.

Vol. 1 : Fasc. 2, issued Jan. 1928.

Sur la représentation conforme d'un domaine limité par quatre cercles tangents sur un demi-plan. Pp. 152-166. V. FOCK. *Ueber geschlossenen zweiseitigen dreidimensionalen Räumen.* Pp. 169-180. W. LWOWSKI. *Sur un problème des ensembles congruents.** Pp. 182-193. M. ZARETSKY. *Sur quelques généralisations des problèmes de Saint Venant et de Clebsch dans la théorie d'elasticité.* Pp. 194-203. G. KOLOSSOFF. *Das Kubische Reziprozitätsgesetz.* Pp. 204-232. A. JOURAVSKY. *Sur quelques inégalités trigonométriques.* Pp. 233-238. R. KUZMIN. *Sur les équations d'hydrodynamique.* Pp. 240-247. N. GUNTHER. *Ueber die Anzahl der Nullstellen der Funktion und ihrer Ableitung.* Pp. 248-255. S. GERSCHGOTIN. *Ueber der Algorithmus der Erhöhung.†* Pp. 257-266. B. DELAUNAY. *Sur quelques séries de polynomes.** Pp. 268-274. V. SMIRNOFF. *Ueber eine Zahlentheoretische Anwendung der Laguerreschen Polynome.†* Pp. 275-280. N. KOSCHLIAKOV. *Ueber einen asymptotischen Ausdruck für die Summe der gebrochen Teile einer Funktion zweier argumente.* Pp. 281-298. M. GELBEKE. *Sur la loi de l'attraction.* Pp. 299-300. N. NERONOFF. *Ueber einige asymptotische Formeln.†* Pp. 313-321. A. DYMMANN.

[* Articles in French. † Articles in German. The rest in Russian.]

Mathematics Teacher. (Yonkers, N.Y.)

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Periodico di Matematiche. (Zanichelli, Bologna.)

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Proceedings of the Edinburgh Mathematical Society. (Bell & Sons.)

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Publicaciones de la Facultad de Ciencias Físico-Matemáticas. Universidad Nacional de la Plata.

No. 81, Oct. 1927.

Análisis Matemático. Vol. ii. *Teorias Generales. Funciones de Más de una Variable.* P. 206. H. BROGGI.

Revista Matemática Hispano-Americana. (Soc. Mat. Española, Madrid.)

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